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Semmelweis
His Life and his Doctrine

Ignaz Philipp Semmelweiss
im Alter von 43 Jahren (1861).

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SEMMELWEIS

His Life and his Doctrine

A CHAPTER IN THE HISTORY OF MEDICINE

BY

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Semmelweis

I.

INTRODUCTION.

"In the history of Midwifery there is a dark page, and it is headed "Semmelweis"! What man could close his eyes to the powerful impression of his book? Even now at the present time there are whole pages of his deductions which might stand in the most modern work. And the annihilating logic of his statistics! We younger men for whom antipathies were unthinkable, to whom the reading of coarse tirades about "genius misunderstood," was only tedious, we often find it incomprehensible that the logical conclusions of the doctrine of infection were nowhere drawn: I mean the local treatment; it was the key-stone of the arch, the crown of the whole structure . . . . The efficient application of disinfection midwifery owes without doubt to surgery: most certainly it ought to have been the reverse. If the conclusions and counsels of Semmelweis had been followed, then the truth of his doctrine would have been demonstrated in the compelling language of statistics, and so perhaps Obstetrics would have stood in the forefront of the greatest advance in Medicine which has been made since physicians and physic came into existence."

Such are the generous but justly appreciative terms in which Fritsch,* then of Breslau, referred to the author of the "Die Ætiologie, der Begriff, und die Prophylaxis des Kindbettfiebers" a quarter of a century since.

The claim which Fritsch made for the Semmelweis Doctrine and its practical applications must be conceded by all unprejudiced men, who are fairly well acquainted with the history of Obstetrics. In the whole History of Medicine we find a clear record of only two discoveries of the highest importance in producing direct and immediate blessings to the human race by the saving of life and the prevention of suffering. These were the discoveries of Edward Jenner and Ignaz Phillip Semmelweis.

---

* "Pathologie und Therapie des Wochenbetts," 1884.

In neither case did the discovery fall from Heaven; in neither was there a grasping of Promethean fire; about neither can we speak of inspiration. The discovery of Semmelweis was possible only for a man who had undergone prolonged and laborious preparation, who had directly observed, and had reflected without preconceptions, whose intellect was kept alert and keen because of the warmth of his human sympathy. The heart of Semmelweis was wrung by witnessing around him the suffering and death of thousands of the miserable victims of some baleful agent, which had eluded the efforts of generations of investigators to comprehend it.

"Consider," says Carlyle, "how the beginning of all Thought worth the name is Love: and the wise head never yet was, without first the generous heart."

Many men in all generations have looked into Nature with their natural vision undimmed by the teaching of pedants: many men have been endowed with clear intellects and hearts full of love for their fellow men, with the enthusiasm of humanity, and they have been enabled to achieve some signal service for the human race in their day and generation; but in the whole history of medicine there is only one Semmelweis in the magnitude of his services to Mankind, and in the depth of his sufferings from contemporary jealous stupidity and ingratitude.

The record of the steps which led up to the establishment of the "eternally true" etiology of puerperal fever is not only of engrossing interest as history, but it must remain of perennial value as an example of the application of logical method in working from the known to the unknown in Medicine. We trace the emancipation from the blinding tyranny of traditional doctrine, and then observe the positive stride from the known to the unknown which marks the final discovery as nearly unique in its magnitude in medical history. Whether it was equalled or excelled by that of Edward Jenner is a question which does not concern us for the present; but in any case there can be no question of the greater human interest, in the pathos and the tragedy, of the Semmelweis story.

Equally interesting are even the biographical details of a too brief, and, apart from the Discovery, an uneventful career. The incidents reveal to us the great personality of the man; we join the band of loyal and devoted friends and supporters who cheered him with their early sympathetic appreciation, and helped him in the fight; and we try to estimate with an equal mind the qualities of the antagonists who embittered his existence and hurried him into the grave. In appraising the character and achievement of Semmelweis and in animadverting on his chief opponents, an effort is made to be guided by the maxim of a great Frenchman: "On ne doit aux morts que ce qui peut être utile aux vivants, la justice et la verité."

The story of the controversy concerning the "Doctrine" is also full of interest, and it is of permanent value from the psychological point of view. We have to contemplate the application of detestable controversial methods: the use of misrepresentation by false suggestion and of insult by disdainful silence, the affectation of exact and encyclopædic knowledge to conceal shallow ignorance, the confident assertion of inaccuracies verging on falsehood, the assumption of official dignity in place of condescension to ratiocination, the nauseating sycophancy of henchmen and aspirants for promotion, the tergiversation, feebleness and inconsistency of superfluous participators in the controversy; and always, opposed to all these uncomely things, patient earnest argument based upon irrefragable evidence, occasionally relieved by a touch of irony or a sarcastic illustration; and through all, the note of wistful appeal for the adoption of measures which would bring to an end the heartless sacrifice of human life.

Deplorable as some of the incidents illustrative of this psychological phase of the controversy may be, its value to teach and to warn can hardly be overrated. When disputes on even minor medical questions arise and participants say rash and harsh things in their perfervid zeal for a party or an issue, no great harm is done: it is when keen self-conscious partisans prepare and shoot poisoned arrows in the library or lecture-room, that they might even at the present time do worse than "Remember Semmelweis."

The story of Semmelweis and his achievements is hardly known in England, even now after the patriotic celebrations in Buda-Pesth at the unveiling of the international statue in 1906. As far as we have learned there is in existence only one biography of Semmelweis in the English language, the short monograph by Dr. Duka, a Hungarian patriot of the Revolution, published in 1888. No attempt has been made at any time to place before the English reader the chief work "Die Ætiologie," or to give fairly full details of the life history of Semmelweis.

Our chief object in compiling this treatise is to bring to the notice of the Anglo-Celtic races, to whom English is the mother tongue, the story of Semmelweis, and to try to do justice to his memory.

Any writing from however obscure a source, which calls strongly and truthfully the attention of the medical profession to the still deplorable ravages of puerperal fever, cannot fail to be in some measure beneficial. That end, however, can be gained by the vast number of contributions to the hundreds of medical journals throughout the world which are always seeing the light, and may ultimately attain the goal which we all devoutly desire. But the story of how the endeavour to eradicate childbed fever originated, with some hope of success, when the "eternally true" etiology of puerperal fever was first revealed to Europe, should serve for inspiration and encouragement to all earnest and thoughtful members of the profession of medicine. An attempt is made here for the first time to convey to English readers an impression of the actual contents of the works of Semmelweis, especially of his chief work, "Die Ætiologie."

Translation would have been a comparatively easy task, but the book is so full of repetitions, largely owing to its construction by throwing together a series of separate articles, sometimes without due regard for sequence and symmetry, that a simple translation would have made tedious and far from attractive reading. Even then biographical and historical material in elucidation would have been essential. Then the statistics: the tables are one of the most striking features of the work and testify to the marvellous industry and earnestness of the author, but the vast mass of them have ceased to interest because they served this purpose long ago; they consequently are now of no value except historically: details may well be forgotten. Their spirit still lives in the conclusions which have found expression in the midwifery practice of the last forty years.

The sequence of events has been followed rather than the original order of the matter in the book, so as to present the contents in a sort of biographical and historical order. Special care has been taken to render justice to the story of the Discovery as given by Semmelweis himself, supplemented and explained occasionally when possible by the statements of his personal friends.

The autobiographical material in the "Ætiologie" is always interesting, but it is rather scanty in amount. The work itself has long been inaccessible even in the German language. This difficulty has recently been removed by the publication in 1905 of the Collected Works of Semmelweis (*Semmelweis' Gesammelte Werke*) edited by Dr. Tiberius von Győry, of Buda Pesth, under the auspices of the Hungarian Academy of Sciences—a patriotic enterprise greatly to the honour of Hungarian Medicine.

An effort has been made in the long chapter on "Some Forerunners and Contemporaries" to settle the question of priority of discovery in certain respects, especially regarding the mistaken and altogether groundless claims put forward in recent years for Oliver Wendell Holmes. Only misapprehension of the scientific points at issue, in the absence of patient study of the history of midwifery, could have made such pretensions possible.

In order to explain the position of Holmes it was necessary to give a short account of the earlier and vastly more important contributions of British Obstetricians, and consequently an attempt is here made to differentiate and to indicate the pre-eminent position of British midwifery in regard to the doctrine of contagion in puerperal fever.

Among the separately published and accessible biographies by personal friends of Semmelweis or by writers who obtained information from friends and contemporaries of Semmelweis, the most important is that of Hegar which appeared in 1882. Hegar had the good fortune to obtain from Professor Tauffer, of Buda-Pesth, a specially prepared translation from the Hungarian of the *Denkrede* of Dr. Fleischer, a former assistant to Semmelweis, delivered in 1872. Hegar's monograph is entitled: "Ignaz Phillip Semmelweis, Sein Leben and Seine Lehre."

Dr. Jacob Bruck, of Buda-Pesth, wrote perhaps the most complete and appreciative of all the monographs. It appeared twenty years after the death of Semmelweis, but much direct evidence concerning the life and character of the man was still available and was judiciously used.

The most recent biography is that of v. Waldheim of Vienna, published in 1905. v. Waldheim's work posseses some valuable features of its own. He is able to give the purport of conversations with the widow of Semmelweis and other elderly persons who could draw upon their memories for biographical details.

v. Waldheim had also the special advantage of being able to peruse official documents belonging to the Ministry of Education, the University, and the General

Hospital, in reference to the Vienna period of the career of Semmelweis. He also draws largely upon the work of Bruck.

These monographs and others, especially that of v. Waldheim, together with much literary material more or less relevant, as well as some personal reminiscences of hospital work in Vienna, are laid under contribution for the purposes of this treatise.

II.

PARENTAGE AND NATIONALITY.

Ignaz Phillip Semmelweis was born about the middle of July 1818, at Ofen or Buda, which Western Europe now recognises as a portion of the great city of Buda-Pesth, the capital of Hungary. He was the fourth son among eight children born into the family. His father was a commercial man or shopkeeper in a good way of business, and his mother was Theresa Müller, daughter of a man in a corresponding social and commercial position.

Since Semmelweis ended his life of suffering and persecution, mainly at the hands of German university professors, and acquired a world-wide celebrity of which any nation might be proud, attempts have been made to claim him as of German nationality, and even specifically as Austrian. The claim is absurd; and it has been completely disposed of in recent years by Tiberius von Gyóry* in his criticism of v. Waldheim's biography. Semmelweis was Hungarian in everything but name. The history of Hungary tells of German colonies planted for generations throughout the length and breadth of the land from Pressburg to Transylvania in the attempts to Germanise Hungary. Instead of becoming German, Hungary largely assimilated the foreign element, and one of the most prosperous of the assimilated colonies was that of Ofen or Buda, the site of a royal palace, and only separated by the Danube from Pesth, the most populous and flourishing of all the Hungarian towns.

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* Semmelweis: "Berliner klin. Wochenschrift," 1905, No. 33.

To claim Semmelweis as an Austrian would be just as fair and reasonable as to claim any distinguished Irishman of Ulster whose forebears had been in the North of Ireland for the generations since the Revolution and "Boyne Water" as a Scotsman, because he bore a Scottish name, or to claim even Parnell or Emmet, the Irish patriots, as Englishmen, because their names were not autochthonous Irish.

Semmelweis was a true Hungarian by birth, temperament and education, and he and his brothers proved in the troublous times of revolution and civil war that they were Hungarian patriots who could make great sacrifices for the Fatherland.

SCHOOL EDUCATION.

In due course the boy Ignaz went to an elementary school. Education was then at a low ebb in Hungary, and the education of Semmelweis was from first to last upon the whole unsatisfactory. The children in a Hungarian-German Community had to make use of the two languages and Semmelweis is alleged, on by no means conclusive evidence, never to have mastered either so as to speak without dialectic accent or to write with facility in the style of a well-educated man. His schools and schoolmasters must be blamed for the deficiencies which he himself called "an innate aversion to every thing which can be called writing." That he spoke German with an accent proves nothing. Some of the most distinguished professors of his time in Vienna lectured with an unmistakeable *Wiener* dialect.

The education of the grammar school or gymnasium was just as defective in its way as that of the elementary school. The pupil was not to blame. He was a clever boy with a ready tongue, full of energy and warmth of heart and imagination which found copious expression, until, with adolescence, he was overtaken by the self-consciousness by which he lost confidence in his capacity for spoken or written language, and he even exaggerated his deficiencies.

The defective school education of the physically strong and restless lad had some advantages: it did not cram him with knowledge, it did not make him prematurely a too sedative and receptive student, and it left him with a natural eye and an unsophisticated mind. He acquired no prepossessions, and he never learned how to bow down before authorities, like so many of the unfortunate young pedants whom he had to encounter as antagonists in after years. It may have been the result of want of mental discipline in early youth, or it may have been the outcome of certain idiosyncracies of intellect and temperament, but one of the salient features in the controversial method of Semmelweis, when fighting for his doctrine, was a want of reverence for the *verba magistri*, and a capacity for going straight to the heart and relevant parts of a question. Defective conventional school education had left his vision clear to see only what was to be seen, and his intellectual faculties free—so that he could think for himself and form independent judgments and logical inductions from the facts of experience.

v. Waldheim is our only authority for the defective and dialectic speech of Semmelweis. But this fact is beyond controversy—when he began to lecture on midwifery in Buda-Pesth, with the choice of three languages in which to address his students, Latin, German and Hungarian,—he employed the Hungarian language, while all his colleagues spoke in German. v. Gyory also calls attention to the fact that his portraits, almost without exception, present him wearing the Hungarian national dress worn by men of his social position.

UNIVERSITY EDUCATION—GENERAL AND PROFESSIONAL.

After two years at the University of Pesth devoted to the study of "philosophy," which no doubt meant the attendance at lectures on literature and the classical languages, including Latin, which was not yet dead, but was still the formal language of the educated Magyar, Semmelweis repaired to Vienna and inscribed his name as a student of law at the University. This was in the autumn of 1837, when he was nineteen years old. But he was disappointed with his start in the study of law, and after accompanying a friend who was a student of medicine to hear a lecture on anatomy, he at once made up his mind as to his future profession; he believed that he was better fitted for the study of Natural Science and Medicine than for Law. So he registered as a student of medicine, and went through the usual courses.

The second and third school-years he spent at the University of Pesth. He attended the routine courses of medical instruction, apparently in a rather perfunctory manner, for no one of the professors of the medical faculty of the time appears to have produced any lasting impression upon him.

The remaining years of undergraduate study (1840—1843) he passed in Vienna. It was then he made the acquaintance for the first time of Klein, Professor of Midwifery and Director of the First Obstetric Clinic attached to the great General Hospital. Semmelweis was a hard-working student and gave much attention to the clinical work of all the departments, but there was nothing asectic about his manner of life. He was always free from pecuniary embarrassment, and he is described by his friends as a bright and jolly companion, and as a student lighthearted but industrious. There is much testimony to the effect that his medical student years were the happy time of his life, testimony perhaps unconsciously exaggerated to bring out by contrast with greater distinctness the gloom that was so soon to overtake him after his great discovery.

On the 2nd of March, 1844, Semmelweis had passed all the examinations for the degree of doctor of medicine, including the disputation over a Latin dissertation entitled, curiously enough, "De vita plantarum." When the day came for the conferring of degrees Semmelweis did not appear, and he sent no written apology. He had been suddenly summoned to Buda to the death-bed of his mother, and that had driven everything else from his mind. After performing the final duties of filial piety he returned to Vienna and obtained his degree in April 1844. He had then evidently formed no plans for the future, except that he was able to sign a declaration that he did not intend to remain in Vienna

For the diploma of Master of Midwifery which had next to be obtained, Semmelweis prepared himself with unusual care, for he had been already attracted to the subject, probably by the personality of Dr. Johann Chiari, assistant in the Obstetric Clinic for Students of Medicine. Semmelweis attended the prescribed course of practical instruction in midwifery more than once, and he and Chiari, who was about the same age, became firm and constant friends. One case which they then observed together throws a singular light upon the state of gynæcology, and illustrates the simple faith of the time in the prevailing epidemic doctrine of the etiology of puerperal fever. A woman, who suffered only from a fibroid polypus of the uterus, was admitted into the Clinic, and the tumour was removed in the usual way. She soon became ill with symptoms similar to puerperal fever, and died. Post-mortem examination shewed the pathological changes in the parts to be identical with those of puerperal fever. Chiari, who had naturally accepted the ordinary opinion taught in the Clinic, explained that the death was due to epidemic influences, which were sometimes so virulent that the puerperal fever attacked even non-puerperal women. Considering what was so soon to happen to both Chiari and Semmelweis, the incident is quite dramatic and worthy of record.

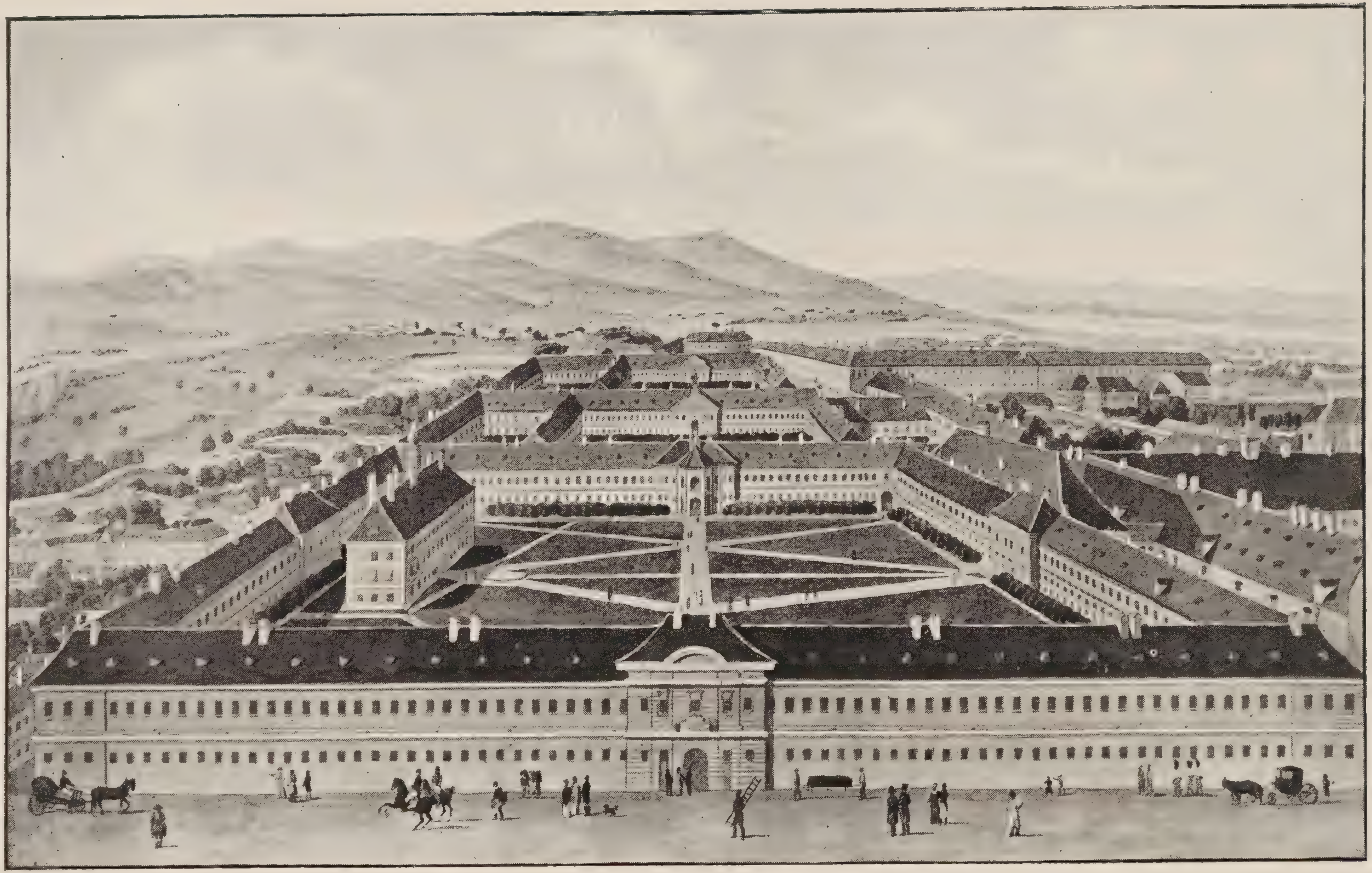
On the 1st of August, 1844, Semmelweis was promoted to the degree of Master of Midwifery. He had by that time decided to devote his professional life to the science and practice of Obstetrics, and he says in the Introduction to the *Ætiologie* :

> "On the 1st of July, 1844, I announced myself to the Director of this Clinic as aspirant for the post of Assistant the first time a vacancy should occur. On

the 1st of July, 1846, I definitively assumed the duties of Assistant at the First Obstetric Clinic, but I had to resign in October in favour of my predecessor, Dr. Breit, who had obtained a two-years' extension of his appointment." . . . This was contrary to the custom prevailing in the Obstetric Clinics. "Dr. Breit was the first to whom this favour was granted."

During the two years between July 1844, and July 1846, Semmelweis, as Aspirant, had free access to the practice of the Clinic without being tied down to the routine duties of assistant. Of the excellent opportunities thus afforded him Semmelweis availed himself to the uttermost. As he had no time to attend the Gynæcological Clinic he began at this period to examine the bodies of women who had died from gynæcological diseases or operations, and owing to the friendly interest of Rokitansky he obtained abundant material.

In order to clearly appreciate the circumstances under which the great discovery was made, it may be desirable to know something of the environment of Semmelweis at the time when he entered upon his official professional duties. What was the scene of his professional occupation? What sort of man was his chief? Who were the friends with whom he associated, and whom he depended upon for advice and encouragement? We should also try to form some tolerably clear estimate of the rather chaotic state of opinion with regard to the causes and nature of puerperal fever prevalent at the time when Semmelweis commenced to make the observations which produced the revolution in professional opinion and practice.



General Hospital of Vienna, 1792.

III.

LIFE IN VIENNA.

The General Hospital and the Lying-in Division. Retrospective.

After Maria Theresa had reached a time of general peace, she devoted the remaining years of her life to the welfare of her subjects—fostering science and art, including medical science and the foundation of hospitals. It was she who laid the foundations of the lying-in hospitals of Vienna and of Milan, and four years after her death her son, the Emperor Joseph, established the great General Hospital of Vienna (Das allgemeine Krankenhaus), including the famous Lying-in Hospital, which was from the beginning the largest of its kind in the world. Special attention appears to have been given to midwifery instruction in Austria both by Maria Theresa and her son in order to make amends for the barbarous past, a time not so remote, when the teaching of midwifery had almost entirely ceased in Austria. Maria Theresa had sent Crantz to the West, to Paris and London, in order to acquire a knowledge of modern midwifery. The object was to enable him to instruct his fellow-countrymen on his return home. Crantz appears to have come under the influence of Levret in Paris, and Smellie in London, hence the prevalence of instrumental delivery in Vienna until the time of Boër.

Joseph II., the autocratic reforming Emperor, continuing the policy of his mother, sent Lucas Johann Boër to France and the United Kingdom to learn the secret of the success of English midwifery practice. Boër, after visiting Paris, worked for a whole year in London, and associated with Denman and his contemporaries. On his return in 1788 he was appointed professor of midwifery and director of the new lying-in hospital.

Joseph II., "after seeing all his undertakings ruined," died in 1790, and left Boër to struggle on for over thirty years of reaction under the most serious discouragements. But he was remarkably successful, and he prevented serious losses among his patients in the lying-in hospital from puerperal fever. It was Boër who introduced the principles of English midwifery into Vienna. In his last year, 1822, as professor and director of the institution, the deaths among 3,066 patients amounted to 26, that is, a mortality of 0·84 per cent.

Boër was succeeded by Klein, a *Höfling,* and former assistant, and perhaps the least fitted of all the candidates for the appointment. His career from first to last appears to have depended upon official favour. Klein weakly consented to teach midwifery even to midwives by demonstrations on the cadaver instead of the phantom, a thing that Boër refused to do, and in his first year of office 237 patients out of 3,036 died from puerperal fever, a mortality of 7·8 per cent.

Such was in brief the earliest record of Professor Klein under whom Semmelweis took service as full assistant in 1846.

Some time about 1840 the Lying-in Hospital had been much enlarged and divided into two Clinics, one for students of medicine, the other for midwives. After the division the results of practice in the two Clinics could be compared, and the mortality in the First Clinic, up to the time when Semmelweis entered upon his duties, was always about three times that of the School for Midwives.

The opportunity of comparing the two Clinics, which seemed to the superficial observer to be identical in every respect, was an important factor in solving the enigma of the cause and nature of puerperal fever. It amounted in some measure to research by experiment.

Semmelweis as Assistant.

In the end of February, 1846, Semmelweis became provisional assistant in the First Obstetric Clinic. Rarely has a man entered upon the responsible practical duties of his profession so well equipped by a ground-work of theoretical and scientific knowledge, as well as by practical experience acquired under supervision. He had been working for two years at least at the pathological anatomy of obstetrics and gynæcology with the help and inspiring counsels of Rokitansky, the world-famed professor of pathology, upon the abundant material supplied by the First Obstetric Clinic; and he also made the autopsies in the Second Obstetric Clinic owing to temporary circumstances. During the same period he had the privilege of assisting in the practical clinical work in the First Obstetric Clinic under Professor Klein and his assistant.

During his "aspirant" stage he lodged near the General Hospital, and no doubt made the acquaintance of many young graduates and students of medicine, and shared with a light heart in the social academic life of the Josephstadt, the Vienna Latin-quarter. Among the many Hungarian students was Ludwig v. Markusovszky, of Buda-Pesth; and between him and Semmelweis a friendship sprang up which continued for the remainder of their lives, and was a source of happiness and mutual advantage, perhaps more especially to Semmelweis in the later years. Markusovszky, of whom we shall hear much, was a singularly able man, and a devoted friend.

After a few months in discharge of the duties of provisional assistant, Semmelweis undertook all the work and responsibility of ordinary assistant. It now became part of his routine duty to visit and examine every patient in the early morning, so as to report to the professor on his arrival in the wards. He was accompanied by a crowd of students, each of whom might examine the patients. Here we have a glimpse for the first time of practices which may well make the modern accoucheur shudder to think of.

In the afternoon it was the duty of the assistant to take the students round for purposes of clinical instruction, and examine every patient who was in labour. He had also to be always ready day or night to perform obstetric operations or to render whatever assistance might be required by the midwives within his division.

v. Waldheim says that there are now four assistants and half a dozen operators for the work which Semmelweis did in those days single-handed. It must, however, be kept in mind that very few obstetric operations were performed in those pre-anæsthetic days, and under the Boër tradition of forbearance and trusting to Nature.

Not satisfied with his obligatory duties, Semmelweis still continued to work at the institute of pathological anatomy in the early mornings before visiting his patients. His chief object in undertaking this extra work was to perfect his knowledge of the pathological anatomy of obstetrics and gynæcology.

Busy and contented as he then was, a shadow soon began to creep over his professional life, which was destined to determine his whole course of thought and action to the end of his days. What was the cause of the frightful mortality from childbed fever among the patients of the First Clinic? In this year 1846 there were 460 deaths in the First Clinic and 105 deaths in the Second from puerperal fever alone, in spite of all the treatment then known in the practice of midwifery. Other young obstetricians, his predecessors and contemporaries in official positions, knew all the facts and regarded them with comparative indifference. In the spare time between professional work and social engagements they could discuss small variations in operative procedure, trifling modifications in the form of instruments, or the introduction of fresh therapeutic futilities, but Semmelweis found no interest in such things. It was under such circumstances, *e.g.*, that Carl Braun, his immediate successor as assistant, invented his blunt decapitation hook, and other assistants made contributions to obstetrics, which are now justly consigned to oblivion.

But Semmelweis was not a man such as these; his intellect refused to accept verbiage for reality; he could not amuse himself with toys; his heart bled for the sufferers whom he saw carried off daily under his hands. The questions which constantly recurred to his mind were: What is puerperal fever? How does it arise? What can be done to prevent it? What treatment can avail to mitigate its ravages?

He devoted his whole time to the study of the malady in the library, in the dead house, at the bedside. His thoughts and actions were all concentrated on the problem; he learned all the professional opinions hitherto accepted at home and abroad; and such conclusions as he reached from clinical observation were in conflict with the prevailing doctrines. "I could not discover in the hitherto prevailing principles underlying the etiology of puerperal fever the actual existence of the alleged etiological factors in the many hundreds of cases which I saw treated in vain."

Doctrines of the Etiology of Puerperal Fever before Semmelweis.

Let us inquire what he had to unlearn and what were the available means of building up some positive system in place of that to be demolished. Semmelweis, like his predecessors in the office which he came to occupy, and like all the professors of midwifery throughout Europe for about two centuries before his time, had been taught the orthodox conventional theories prevailing in his generation. His own professor of midwifery was Klein who had been a pupil and assistant of Boër. With regard to puerperal fever, Boër's teaching had been in some measure different from that of his Continental contemporaries inasmuch as he had been influenced by the English doctrine of contagion, and had adopted the milk-fever variety of contagionism.

In order to appreciate the nature and extent of the Semmelweis revolt it is necessary to form some conception of the notions prevalent at the time when he began to doubt. These opinions are so various within limits and apparently so confusing that they are difficult to state clearly and succinctly. Among the best statements, comprehensive and discriminating, are those of Hegar in his work on Semmelweis, of v. Winckel in his "Pathologie und Therapie des Wochenbetts" and of Fehling in "Die Physiologie und Pathologie des Wochenbetts."

All these and other authors are here laid under contribution in an attempt to state as concisely as possible an exasperatingly complicated subject.

According to the most generally accepted doctrine there were two etiological factors: one external, acting from without, the other internal, depending upon the condition of the organism during pregnancy and parturition; and whatever changes of nomenclature were introduced and whatever more or less obscurely expressed opinions became most prominent and generally accepted in a generation, we always find these two etiological factors at work in producing the phenomena of the malady.

Willis (about 1662) was the first in modern times to write the term *Febris puerperarum;* and Strother in 1718 translated it into Puerperal Fever.

To go no further back than the middle of the eighteenth century we find a school of believers in anomalies of the lochia. Among British teachers of the doctrine of Lochial Suppression as the central fact in puerperal fever was Smellie. He probably brought it from France where it was taught in the School of Mauriceau, and his influence as a teacher spread it over Western Europe.

To many prominent teachers puerperal fever was a milk-fever. By it they explained serious symptoms as milk-pneumonia, milk-peritonitis, and so on. One French observer actually found milk in its natural form within the peritoneal cavity! This theory had many supporters in England, and that is probably why it was adopted by Boër and taught with some modifications in the Vienna School, at least up to the end of the first third of the nineteenth century. Boër's successor, Professor Klein, was incapable of evolving anything original, hence in all probability Semmelweis as a student was taught Boër's theories of the etiology of puerperal fever.

The physiological school,—*lucus a non lucendo,*—held to a mixture of the lochial and milk theories, which was too ingenious to take any considerable or prolonged hold upon the professional mind.

The gastric-bilious fever theory had a considerable vogue in England for a time. Among its advocates was Charles White, of Manchester, and afterwards Denman. The severer cases were for them a putrid fever.

Arising about the same time and holding its ground for about a century was the theory that inflammation was the central fact in childbed fever. This school naturally split itself up according to the organ chiefly affected in the opinion of the individual "authority." It was chiefly metritis or peritonitis or an inflammation of the intestines or even of the omentum. William Hunter and later Baudelocque were perhaps the most prominent and influential advocates of the theory of peritonitis. It was believed by some who accepted peritonitis as the central fact that epidemic and endemic influences complicated the peritonitis with septic metritis and metrophlebitis.

About the early middle of the eighteenth century we find the first references to erysipelas as closely associated with puerperal fever. Later, when observations were made which could be explained only on the theory of contagion, as then understood, the belief in the intimate connexion between erysipelas and puerperal fever was almost universally accepted, especially in Great Britain and Ireland. Gordon, of Aberdeen (1795), alleged that puerperal fever was an erysipelas of the bowels and peritoneum. By the middle of the nineteenth century the almost universal belief in England and America was that erysipelas in the puerpera and puerperal fever were identical maladies. It should however always be remembered in judging of the evolution of opinions and of the men who held them, that in England and largely in America the term erysipelas, "the rose," was applied to any reddening of the skin in any part of the body in the puerpera. Such appearances, first explained by

Hervieux, we now recognised as local expressions of sepsis.

A doctrine maintained all this time, although apparently inconsistent with the theory of an intimate relationship with erysipelas, was that puerperal fever was a species of disease *sui generis,* independent as an entity, but the most variable in its phenomena owing to fortuitous influences. And this belief still prevailed, especially in England, when all the zymotic diseases were supposed capable of assuming the form of puerperal fever while concealing even their most characteristic features. For example scarlet fever in the guise of puerperal fever without angina, without rash, "Nulla febris est quae non aliquando cadat in puerperam." This was the "variable" theory of puerperal fever as enunciated by Eisemann in 1837, and it gave a certain added air of originality to his over-rated work.

Ultimately came the dawn of the wound-fever theory as the internal factor, but even in England where the most rational pioneer opinions had developed, there was a general belief in a *contagium,* and the pioneers of our modern pathology of the disease harked back upon a "specific something," an "unknown something," the *divinum aliquid*, perhaps a specific primary change in the blood, producing the local lesions known to the pathological anatomist.

At the early period when this chaos of notions prevailed with regard to the internal factors in the etiology of puerperal fever, the external factors were still more vague and even incomprehensible.

As external factors were certain alleged influences of an atmospheric, cosmic, telluric kind, to which the term *genius epidemicus* was applied; mere changes in the weather are constantly referred by some writers, especially in France and Germany and even in Edinburgh, as sufficient to account for the greater or less prevalence of puerperal sickness. Others maintained that as the result of such changes a special injurious entity developed and spread through the atmosphere, a miasma. The genius epidemicus might extend over many regions, or it might be restricted to a narrow area, even to an individual town. A miasma might be developed and remain isolated in a district or a separate building such as a hospital, and it was specially prone to develop if there was overcrowding of pregnant and puerperal women. This was the theory to which Cruveilhier lent the weight of his authority. Denman was among the first in England to express the opinion that puerperal fever was conveyed infection, but his views are by no means clearly expounded.

Further, it was believed by many that with a certain intensity and extension of the malady a contagion was brought into existence. Here the ancient dogmatic conception of contagion was maintained. According to this theory a contagion represented a specific virus which could take its origin only in the diseased organism, and from that when conveyed by propinquity or actual contact with another individual could produce in that individual the same disease. In some minds the contagium assumed the form of a mysterious halo or areola which clung to the unfortunate practitioner who came under its malign influence.

Coming nearer to the time when the doctrine of Semmelweis was announced, we find an almost general prevalence of the belief that the pregnant and puerperal woman was a thing unique in nature. This last theory of loaded blood and tissues generally, unstable equilibrium on the verge of fever, when even at its best especially vulnerable by the external factor, held the field for long, especially among the older unteachable obstetric mandarins in England; but it had to be completely abandoned in time, and it was the last barrier to the spread of the modern pathology of puerperal fever.

Latterly then, with regard to the internal etiological factor, consideration was given at one time more to some modification in the composition of the blood produced by pregnancy, parturition and the puerperium, at another time more was heard about changes in the solid portions of the body, especially those of the sexual system.

Under the influence of the doctrine of crasis the opinion was formed according to which the special change in the blood, the increase of fibrin, which occurred to the pregnant woman, had no limit fixed to it, and it might so increase that finally deposits in the form of exudation on the inner and outer surfaces of the uterus were produced spontaneously.

From the hyperinotic condition a still further increase might develop, resulting in pyæmia or even in a blood-crasis, a spontaneous "blood-dissolution."

Among the supporters of a theory of spontaneous origin we find, for example, Virchow in 1861 and Barnes in 1875. This condition might also be produced by the direct influence of the external factor, the miasma.

The peculiar anatomical condition of the sexual organs brought about by pregnancy and parturition produced a *locus minoris resistentiae*. This assumed fact explained the frequent occurrence of deposits or secretions from the altered blood, or the influence upon those parts resulting from the miasma, which was taken up from the atmosphere and kept circulating in the blood.

It was also taught that the injurious material could find its way into the sexual organs themselves, and there directly produce the lesions contributing to puerperal fever, or completely create the fever after first poisoning the mass of the blood.

Besides all this, the opinion was still quite generally entertained that all the anatomical alterations peculiar to puerperal fever arose idiopathically owing to injuries, bad contraction of the uterus, chilling, errors of diet, emotional conditions, and that then the blood-changes such as are present in every form of fever might be secondary results.

In the opinions concerning the relationship of the external noxæ, of the composition of the blood, of the peculiar anatomical changes in the puerperal sexual organs, and of the pathological-anatomical observations, we find manifold modifications and variations among the various authors. The differences of opinion arose essentially from this circumstance that one author would attach more weight to one factor, another to some other factor, and then enunciate as an "authority" some half-digested theory, which was sure to be accepted in some quarter, usually in his own school or university.

At this later period, only two points are worthy of special attention: (1) the relations of puerperal fever to the various acute infectious diseases, and (2) the relations in which it stood to the old pyæmia of the surgeons.

Whilst in Germany and France the contagiousness of puerperal fever was the subject of much controversy, and was as a rule rejected, it was universally accepted in England. As a result of much observation, even in private practice, the conclusion was reached that scarlet fever, measles, smallpox, erysipelas, pseudo-erysipelas, even typhus and typhoid fever, stood in intimate relationship to childbed fever. It was believed that the virus of each of these diseases could produce puerperal fever in the peculiarly sensitive lying-in woman. The usual notion of this relationship was that the poison of these diseases could, owing to the peculiar quality of the blood and tissues of the puerpera, as on another soil, produce quite different phenomena from those produced under ordinary circumstances in the woman in normal conditions of health exposed to infection. The further conclusion was not far to seek, that for puerperal fever, generally speaking, there existed a specific virus, and many identified this virus with that of erysipelas or pseudo-erysipelas.

The relation of puerperal fever to pyæmia of the old surgeons, or surgical fever, offers for our consideration quite peculiar phases. Hegar says that Cruveilhier was the first to compare the puerpera to a wounded person who had become affected by a miasm arising in a hospital, and that Eisemann next referred to the injury to the inner surface of the uterus, and its importance as the point of entrance of the miasm or contagium, the *locus minoris resistentiae*, for the noxa or injurious agent absorbed by the lungs and circulating in the blood. But the opinion that the placental site must be considered a wound surface was prevalent in England at least a quarter of a century before Cruveilhier.

Generally speaking, as yet the conception of the identity of childbed fever with pyæmia was still far from the minds of the obstetricians at the time when Semmelweis began to try to solve the problem of the appalling mortality in the Vienna Lying-in Hospital. Puerperal fever was considered to be as a disease something peculiar to pregnancy and the puerperal state.

The great difference between Continental and British opinion was that in Continental Europe more consideration was given to the *genius epidemicus* before which men could only helplessly bow their heads; how could they fight an "atmospheric cosmic telluric influence?" In Great Britain and Ireland the mysterious *contagium* received most attention; it was believed that the contagion could be destroyed, and that belief produced a most anxious and remarkably effective prophylaxis.

In the United States of America there was no theory of the etiology of puerperal fever of native growth; the chief teachers of midwifery refused to believe in either epidemic influence or contagion, and the medical practitioners, including O. W. Holmes, who did accept the contagion theory, borrowed their working hypothesis almost entirely from the writings of English obstetricians.

Such then were some of the opinions with regard to the causes of puerperal fever which Semmelweis would have to learn in preparation for that examination for the degree of Master of Midwifery which he passed in 1844. From July, 1844, as Aspirant, he had the opportunity of examining patients under the direction of Breit, the assistant in the First Clinic, and he heard the theoretic opinions and sought for the expected phenomena in the individual case. But his efforts to reconcile the clinical phenomena with the etiological factors entirely failed, and the failure produced the watchfulness and exact observation in the lying-in wards and in the postmortem room which led to the Discovery.

Semmelweis, beginning as a milk-fever epidemicist, had much farther to travel to the doctrine of a decomposed animal organic matter directly conveyed to the genitals of the individual patient as the only etiological factor, than, for example, the British contagionist. When his discovery had completely seized him he was able to place erysipelas in its proper position in relation to puerperal fever, as merely a producer of decomposed animal organic matter. It was not until 1874 that we find Matthews Duncan inculcating for the first time on British listeners the same opinion.

The modern universally accepted doctrine of the etiology of puerperal fever is that puerperal fever is wound-fever, and wound-fever is wound-poisoning. The supplementary doctrine is also universally established except for a mere logomachy, viz., all infection comes from without.

In the works of Semmelweis we find all this implied, sometimes almost explicitly stated.

How he came to bridge the space between the theories which he was taught and the modern doctrine must now be our task to trace.

Friends of Semmelweis.

But first let us glance for a moment at the social surroundings in which the work of Semmelweis was carried on, and try to take some impressions of the immediate associates who influenced his career at the time by placing opportunities in his way, and stimulating him by the example of fruitful achievements.

Semmelweis was remarkably fortunate in his early friends in Vienna, and that fact is perhaps a high testimonial to his own intellect and character—the clear head and kindly heart.

Chief among the group of singularly able but as yet comparatively little known men, who were beginning to attract the attention of the medical world of Europe to the Vienna School of Medicine, were Rokitansky, Skoda and Hebra. Each of them was to become world-renowned in his own special department of medicine, but they were destined largely to perpetuate their fame by association with the genial and vivacious young Hungarian whom they accepted within their circle of medical reformers, and befriended on many occasions when friends and counsel were sadly needed.

At the time when Semmelweis began the study of medicine Rokitansky had already been working at pathological anatomy, almost as a pioneer, at the Vienna General Hospital for nearly twenty years, and he had been appointed Professor of Pathological Anatomy. He was then about forty years of age, and was a recognised authority in his subject, like Virchow somewhat later. He afforded Semmelweis many facilities for the study of gynæcology by post-mortem observations when he was a waiting aspirant, and Semmelweis continued to work hard at the subject even after he received the official appointment as Assistant to the Professor of Midwifery.

There was a dignity and power about Rokitansky which compelled to progress even the dull crowd of Höfling placemen in the Medical School, who resisted all advancement in medical science.

Skoda, who was then a man of thirty-nine, could look back already upon a somewhat chequered career. When a junior physician at the General Hospital he had published the work on Auscultation and Percussion which became so famous afterwards. As usual with pioneers he became an object of derision and the butt of the obscurantist seniors and sycophantish juniors of the medical department, and all sorts of difficulties were placed in his way. The despotic director of the General Hospital had his mind poisoned against Skoda, and on the wretched pretext that he hurt the patients and made them worse with his thumping and pressing on their chests, he was transferred to the Lunatic Asylum to practise there! His position became intolerable in his new position. The future great diagnostician, who was not well off, accepted the post of police surgeon. But Skoda had some powerful friends, and better times were dawning upon the Vienna Medical School. In 1840 a special department for Diseases of the Chest was formed in the General Hospital, and Skoda was placed in charge of it in spite of the opposition of his old enemies.

Next year Skoda was also put in charge of the new division for Diseases of the Skin, with Ferdinand Hebra, a young man of five-and-twenty, as his assistant. Thus was founded the department of Dermatology for which the Vienna School of Medicine ultimately became so famous. To these three men was largely owing the rapid growth in fame and fortune of the Vienna School.

Semmelweis was a diligent student of all the subjects taught by these ardent medical reformers, and especially of pathological anatomy. He had also to attend the lectures of Professor Klein, who appears to have been a dull man and a poor teacher. From him Semmelweis was destined to suffer much more than from tedious discourses.

Klein had been, as we have seen, an assistant to Lucas Johann Boër, the greatest obstetrician of his time. Boër himself had been made professor by the great reforming Emperor Joseph II. in 1789, and had maintained English methods of midwifery practice to the end of his official life as a teacher. Boër was detested by the reactionaries and clerics who acquired enormous influence in Vienna after the fall of Napoleon. Courtiers and clerics had constantly tried for years to put favourites of their own in the place of Boër, and they were too impatient to wait for his resignation or death; so, on the pretext of insubordination because he refused to teach midwives on the cadaver instead of the phantom, they deposed him and put Klein in his chair. This was in 1822. We have seen that in the last year of Boër's directorship of the lying-in division of the hospital the mortality from puerperal fever was 0·84 per cent.; in Klein's first year it was 7·8 per cent., and it remained more or less high during his whole tenure of office. He was always opposed to reforms in methods of practice and of instruction even before the time of Semmelweis, and denied that he was in any way responsible for the success or failure of his teaching. Such was the man for whom fate found an assistant apparently suitable for the quiet conventional routine hitherto pursued. The aspirant was a young vigorous man of eight-and-twenty, somewhat florid, stout and prematurely bald; he was frank and smiling, with something almost puerile in his address, and at that time he spoke with just a suspicion of a Hungarian brogue.

Klein had, of course, noticed Semmelweis as a specially industrious and quiet unobtrusive student, and he readily assented to his nomination to the appointment in the ordinary course of administering his department; but the subordinate and coadjutor was one of the most remarkable men in the medical profession of Europe at the time. He was possessed of that type of intellect which refuses to accept phrase for fact, and ancient conventional formulæ and lies for reasoned conclusions. With the kindly sympathies of a strong man and the tender heart of an adult woman, he was to exhibit in due course the most explosive indignation and sarcastic contempt for knaves and fools among students and nurses and midwives, because by their carelessness and levity they endangered the lives of the poor women consigned to their care, and at the same time obstructed the spread of his "Doctrine."

It was largely, as we shall see, to untiring watchfulness and personal example of sacrifice of rest and comfort that Semmelweis owed the remarkable success of his prophylaxis in Vienna and in Buda-Pesth, when want of exacting and constant watchfulness, and lack of conviction, led to complete or comparative failure at most of the lying-in hospitals elsewhere.

FOUNDATIONS OF A NEW ETIOLOGY.

We are now in a position to understand the circumstances under which the discovery was made and to follow the Semmelweis narrative of the *Ætiologie*. It should always be remembered that the *Ætiologie* was written fully ten years after the discovery was first announced, and it was published just ten years after Semmelweis had returned to his *Vaterstadt,* and had ceased to communicate with all the old friends and opponents in Vienna.

The great Lying-in Hospital of Vienna is divided into two portions, called the First and the Second Division.

From October, 1840, the students, both men and women, were taught separately in the two divisions. Before that time students of medicine and midwives received clinical instruction in equal numbers in both divisions. The statistics obtained during the years of instruction in common form an important element in the experimental evidence by concomitant variation of the etiology of puerperal fever, which had to be developed later in the history of the hospital.

Since the division of the hospital into two parts, with medical students restricted to one division and midwives to the other, the mortality from puerperal processes had shown a remarkable difference between the First Clinic and the Second. This fact is brought out in a remarkably striking way by the first table of statistics prepared by Semmelweis:

Table No. I.

	FIRST DIVISION.			SECOND DIVISION.		
	Cases.	Deaths.	Per Cent.	Cases.	Deaths.	Per Cent.
1841 ...	3,036	237	7·7	2,442	86	3·5
1842 ...	3,287	518	15·8	2,659	202	7·5
1843 ...	3,060	274	8·9	2,739	164	5·9
1844 ...	3,157	260	8·2	2,956	68	2·3
1845 ...	3,492	241	6·8	3,241	66	2
1846 ...	4,010	459	11·4	3,754	105	2·7
	20,042	1,989	9·92	17,791	691	3·38
			say 9·9			say 3·3

Thus we see that over these six years the average mortality in the First Clinic was three times that of the Second.

Great as is the difference here shown, it was really greater because of the custom of transferring severe cases of puerperal fever from the First Division to some part of the General Hospital, where they died. The patients were not then registered as dying from puerperal fever.

Le Fort, the friend of Tarnier, who visited the hospital the year before Semmelweis died, says in his remarkable book, *Des Maternités* (p. 151), in reference to puerperal fever in Vienna, "High as the figures are they are, however, still below the reality, for it is almost certain, according to Prof. Späth, that they do not include the women suffering from puerperal fever who were transferred to the General Hospital and died there."

In the Second Division few patients were transferred to the General Hospital: only one now and again considered too dangerous to the other patients, such as cases of smallpox and severe forms of venereal disease.

This high mortality in the First Division as compared with that of the Second, was contributed to by many hundreds of patients whom Semmelweis saw dying from puerperal processes of which, as has been said, he could not detect the etiological factors according to the generally received theories.

In order to convince the reader that this higher mortality could not be explained by the usually accepted etiology, Semmelweis proceeds to pass in review the hitherto recognised etiological factors in the production of puerperal fever.

In the first place, the frightful devastation wrought by childbed fever in the First Division was confidently attributed to epidemic influences. By epidemic influences we alleged that we understood some not exactly defined atmospheric-cosmic-telluric changes which often spread over whole districts of country, and cause puerperal fever in women disposed to it by the puerperal state.

Well now, if the atmospheric-cosmic-telluric conditions of the city of Vienna are of such a nature that they can cause puerperal fever in individuals disposed to it by reason of the puerperium, how does it happen that the atmospheric-cosmic-telluric influences have for a long series of years preferably carried off the predisposed occupants of the First Obstetric Clinic, while at the same time in Vienna, and in fact at the same time under the same roof, women predisposed owing to the puerperium but occupying the Second Obstetric Clinic, have been spared in such a remarkable manner? This fact appears without doubt to imply that if the devastation wrought by puerperal fever in the First Obstetric Clinic is to be attributed to epidemic influences, the same influences must with very slight variations (Schwankungen) recur in the Second Division, else are we driven to the absurd conclusion that the epidemic influences are subject in their destructive activity to remissions and exacerbations every twenty-four hours, and that these remissions have for years coincided with the time of admission to the Second Division, and the exacerbations have for a similar series of years come on at exactly the time for admission to the First Division.

Even if we were to accept such an absurdity as explanation, the difference in the mortality of the two divisions cannot be rationally attributed to epidemic influences. The epidemic influences act during the exacerbation upon the individual either before her admission to the Lying-in Hospital, or they act during her residence in the hospital. If they act upon the individual before her admission to the hospital, then both the patients who come to be admitted to the First Division and those who come into the Second Division, have been subjected to the same destructive epidemic influences, and the great difference in the mortality is not explained. If the epidemic influences work upon the individuals during their residence in the hospital there cannot be such a great difference in the mortality, inasmuch as the two divisions which lie so near to one another as to have an ante-room in common, must necessarily be subjected to the same atmospheric-cosmic-telluric influences.

It was the consideration of these facts which compelled me to the settled conviction that it was no epidemic influence which produced the shocking ravages among the lying-in women of the First Obstetric Clinic.

After this conviction was formed immovably and had completely mastered me, many reasons were soon discovered which still further confirmed my belief. Let us take them in turn :—

If the atmospheric influences of the city of Vienna produce a puerperal fever epidemic in the Lying-in Hospital, it would be impossible that the people of the city, subjected to the same influences, could escape an epidemic among their lying-in women; but as a matter of fact when the fever was raging at its worst in the Lying-in Hospital there was hardly ever a case heard of in the city or surrounding country.

When a cholera epidemic broke out it was well known that the people sickened of the malady both in the hospitals and throughout the city.

A very common and successful method of diminishing the ravages of a puerperal fever epidemic is to close the lying-in hospital. The hospital is not closed in the expectation that the patients will die somewhere else, but because of the belief that if they remain in the hospital they will die owing to the epidemic influences at work within the hospital; whereas if they are confined outside away from the hospital, they will remain well. Hence it follows, as demonstrated, that we act as if we believed that we have not to do with an epidemic disease but with an *endemic* disease, that is to say with a disease resulting from causes which are restricted in their action within the bounds of the hospital.

What would the defenders of the epidemic theory say if any one were to propose that with the object of mastering an epidemic of cholera, the best method would be to shut up the cholera hospital? Again, the puerperal fever, which is the result of traumatism, *e.g.*, a forcible application of the obstetric forceps, is in its course and in the resulting anatomical changes exactly the same as are observed in the so-called epidemic cases. Can it be possible then that there is another epidemic puerperal disease of traumatic origin?

Epidemics of zymotic disease show year-long intermissions; childbed fever, on the other hand, has prevailed in the First Obstetric Clinic for a long series of years with little or no intermission whatever. Does cholera ever remain epidemic for a whole year at a stretch?

If the so-called epidemic childbed fever were really due to atmospheric influences, it could not occur in every season of the year and under the greatest variety of climate; but as a matter of fact epidemics of childbed fever are observed at all season of the year, in the most different climates, and under all sorts of weather conditions.

We shall, in order to prove this point by means of figures, use Table No. 1 once more—adding the first five months of 1847. It shows that in every month of the year there is a favourable and also an unfavourable condition of health in the First Obstetric Clinic. In November, 1841, 53 women died out of the 235 confined, that is, 22·55 per cent. In January, 1842, out of 307 patients confined 64 died, that is, 20·84 per cent.

Portion of Table No. II.

January: Lowest death-rate was in 1847, viz., 10 in 311, that is, 3·21 per cent; highest was in 1842, viz., 64 in 307, that is 20·84 per cent. (other months follow).

June: Lowest death-rate was in 1844, viz., 6 in 224, that is 2·67 per cent; highest was in 1846, viz., 27 in 266, that is 10·15 per cent. (other months follow).

October: Lowest death-rate was in 1844, viz., 8 in 248, that is, 3·22 per cent.; highest in 1842, viz., 71 in 242, that is, 29·33 per cent. (table completed).

So we see that the destructive influence of the epidemic is not restrained by any season of the year; it rages in the severe cold of winter and in the oppressive heat of summer with equal violence. But the epidemic influence is not impartial inasmuch as it does not swing its scourge over all lying-in hospitals alike; it spares some and rages without pity in others, and it goes so far in its partiality that it invades with very different results the separate divisions of the same institution.

It is a fact that lying-in hospitals where instruction is not given, or where only midwives are admitted for training, present with few exceptions better results than those which are devoted to the education of students of medicine.

Table No. I. shows how different were the results as regards mortality in the two divisions of the same hospital. At Strassburg,* under exactly analogous conditions, the results were exactly similar.

"It was on these grounds that I came to the conclusion, and became increasingly stronger in the conviction, that the high mortality in the First Obstetric Clinic was the result of injurious influences originating and operating within the bounds of the First Clinic itself."

If we were to critically examine the hitherto accepted endemic causes of this disease and the alleged causes of the mortality in the two divisions of the great Maternity Hospital of Vienna, we must conclude that either no difference could exist, or that there must be actually a higher mortality in the Second Division.

If overcrowding, as has been alleged, were a cause of mortality in the First Division, then it must have produced a higher mortality in the Second Division, for it was always more crowded than the First Division. The evil reputation of the First Obstetric Clinic drove the women in large numbers to seek admission to the Second; and it often happened that the First Division had to be filled by overflow because there was not an available bed in the Second. In spite of all this the mortality in the Second Division remained remarkably lower than that of the First. Whatever the cause of this difference in mortality it was clearly not owing to the relative degree of overcrowding in the two divisions.

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* Arneth.

We next find a long series of statistical tables bearing on this question of overcrowding (*Ætiol,* p. 13 *et seq.*). Table III., IV., V., VI., VII., etc., up to Table XIII., p. 31, all illustrate the question of overcrowding, and demonstrate that there was no sort of relationship between the number of patients confined within the Division and the mortality.

When the births were at the fewest the number of deaths in proportion did not diminish. With diminution in the degree of overcrowding there was no corresponding diminution in the mortality. This is shewn month by month in detail during the seven years 1841—1847.

When we compare month by month, for 76 months tabulated, the relative mortality and mark its relative diminution, we see nothing in the mortality corresponding to the relative decrease in the number of births, and no relation in the larger or smaller mortality to the amount of overcrowding. Well might Fritsch speak of the annihilating force of the statistics of Semmelweis.

An opinion prevailed that the very building in which so many thousands of women had gone through parturition and the puerperium, had been attacked with puerperal fever and had died, must be so pestilential that it was not to be wondered at if puerperal fever got the upper hand and was perennial. If that were really the cause of puerperal fever then the Second Division ought to have had a higher mortality than the First, because even in Boër's time frightful epidemics had raged in what is now the Second Division, and this before even the foundations of the First Division had been laid.

The *evil reputation* of the institution was such that the women admitted entered it with fear, and that *dread* was declared to be a cause why the patients sickened and died. "That they were afraid of the First Division there was abundant evidence. Many heart-rending scenes occurred when patients found out that they had entered the First Division by mistake. They knelt down, wrung their hands and begged that they might be discharged. Lying-in patients with uncountable pulse, meteoric abdomen, and dry tongue, only a few hours before their death, would protest that they were really quite well, in order to avoid medical treatment, for they believed that the doctor's interference was always the precursor of death. In spite of all this I could not convince myself that fear was in any measure a factor in producing the great mortality in the First Division, because as physician I could not see how fear, a psychic condition, could produce such anatomical changes as those seen in puerperal fever. Besides it must have taken a long time of raised mortality before it could become an impression among the people that the death-rate in one division was persistently very much higher than in the other. *Fear does not explain the beginning of the mortality* ("Durch die Furcht wird der Beginn der Sterblickeit nicht erklärt").

Certain *religious observances* were also accused of increasing the mortality.

The chapel of the lying-in hospital was so situated that the priests, bearing the last sacrament to the dying, could in the Second Division reach the ward where the patient lay without passing through the other rooms, but in the First Division they must pass through five wards before reaching the sick-room beyond. It was usual for the priests, arrayed in their robes, with an attendant marching before them ringing a bell according to Catholic ritual, to proceed to the sick woman to administer the sacrament. According to ordinary arrangement this should be done only once in 24 hours, but in childbed fever 24 hours is a long time, and the priests had to be sent for occasionally in a few hours after their regular visit. It is easy to imagine the impression which the fateful tolling of the bell would produce upon the lying-in women. "Even to me myself it had a strange effect upon my nerves when I heard the bell hurried past my door; a sigh would escape my heart for the victim that once more was claimed by an unknown power. This bell was a painful exhortation to me to search for this unknown cause with all my might. Even to this difference between the arrangements of the two Divisions was attributed the higher mortality of the First Division."

"During my first term of office I appealed to the sense of humanity of the servant of God, and without difficulty it was arranged that for the future the priests would take a roundabout route, without ringing the bell, so as to reach the sick chamber in silence and unobserved. The two Divisions were made similar in this respect, but the difference in their mortality still remained."

It was alleged that the reason for the great mortality was because patients were unmarried women of the most hopeless class of the community, accustomed to earn their bread in want and misery, and amidst conditions which produced great and constant depression of spirits. If this had been the cause of the mortality it would have been as great in the Second Division, for to it exactly the same class of patients were admitted.

It was alleged that in the First Division the medical students examined the women in a coarser and rougher way than was the practice in the School for Midwives. But what is the introduction of the index finger into the wide and long vagina of a pregnant woman, as a cause of injury, compared with the process of parturition? If examination could produce a fatal injury it would be inconceivable that the passage of the body of the fœtus through the genital tract could be otherwise than fatal in its consequences.

The higher mortality of the First Division was ascribed to the wounded modesty of the poor women going through the process of parturition in the presence of men. Most of the patients in the First Division certainly suffered from fear, but not many were troubled with a sense of shame. Besides, it is impossible to conceive how a sense of shame could be an etiological factor in puerperal fever; it could not bring decomposed matter from without nor produce a decomposed matter within the individual herself. Truly, it shows with what want of thought the whole question of the etiology of puerperal fever has been discussed when the persons, who at times are depicted as the most abandoned of the population, have attributed to them in the next sentence a tenderness of modesty such as the upper and highest classes of the community do not claim. Among the upper and even the highest ranks of society labour is conducted by physicians, and their patients do not die of puerperal fever in consequence of wounded modesty in the same proportion as is alleged of the inmates of the lying-in hospitals who, for the sake of argument, are often depicted as the most loose and abandoned of the community.

It was not owing to the *medical treatment* that more patients died in the First Division, because the medical treatment in both divisions was identical. From time to time efforts had been made to obtain better results by transferring all the sick puerperæ to the General Hospital, but they succumbed there also under the most varied kinds of medical treatment.

In both divisions of the Lying-in Hospital the obstetric treatment was according to the principles of Boër, consequently in hardly any case, except in obvious abnormalities, was an operation of any kind ever resorted to.

It was declared that a cause of the mortality in the First Division was that the patients got up too soon after labour. It was usual for them to rise a few hours after the completion of labour, and walk to the beds allotted to them in the lying-in ward, which might be at a considerable distance from the labour-room. Weak patients and operation cases were carried, but the patient after normal labour had to walk. But the arrangements in this respect in the Second Division were exactly the same, except that slightly more inconvenience was caused to the patients in the Second Division. In both Divisions the same arrangements prevailed with regard to the patients leaving their beds at the end of seven or eight days. Very few sickened after that time. A second change of room appeared to make no difference in the health of the patients, and besides, the arrangements were the same in both Divisions.

It is a curious episode, this allegation of danger from early rising in the puerperium. Charles White, of Manchester, in his Treatise, "Of the Causes and Symptoms of the Puerperal or Childbed Fever," published in 1773, claims that the immunity of his patients from puerperal fever depended upon his practice of making them rise within the first day of the puerperium; he never lost a case among his own patients.

And, again, the treatment of a century and a half since has recently come into favour in Germany, chiefly under the influence of Professor Krönig of Freiburg.

Ventilation, or rather the want of ventilation, was blamed by some whose opinions were freely offered. Ventilation was effected in the First Clinic by opening the windows, even in the winter time. But exactly the same method of ventilation was employed in the Second Division.

The washing was blamed by some, because it was said to be mixed with the "wash" of the General Hospital, overlooking the fact that this was also the case with the washing of the Second Division.

Neither chilling nor errors of diet could be laid hold of in explanation of the difference in the mortality of the two Divisions. The method of warming was exactly the same in both Divisions, and the possibility of chilling was equal in both. The food was supplied by the same caterer; the diet regulations were identical in both Divisions.

These were the endemic causes to which was ascribed the great mortality among the hospital cases as compared with the mortality outside the Lying-in Hospital; but they did not throw light on the greater mortality of the First Division, while the same unfavourable endemic influences were at work in a more active form in the Second Division than in the First.

The unfavourable position of the Lying-in Hospital was blamed because of its association with a great general hospital, although in this respect both Divisions had everything in common; they had one common anteroom for both and the construction of both Divisions was the same in every respect.

The disadvantages of uninterrupted clinical instruction, the communication of the rooms reserved for infected cases with the ordinary lying-in wards, the free intercourse of the nursing staff attending the fever cases with those in attendance on the normal puerperæ, alleged to be factors in producing the mortality, were matters common to both Divisions.

Yet since the time when the First Division was devoted entirely to the teaching of students of medicine the mortality there had always been notably greater than in the Second Division.

"Since neither the epidemic nor the hitherto prevailing endemic influences explain in any way the greater mortality of the First Division, let us try to test the value of some other alleged causes of puerperal fever.

"By some recent investigators conception was set down as one cause owing to some metamorphoses produced by the *sperma virile,* and some partly unknown changes of the blood. But I presume I fall into no error when I state the opinion that with the women who have borne children in the Second Obstetric Clinic a conception had previously occurred.

"Whence then the difference in the mortality of the two Clinics? What about the other alleged factors? Hyperinosis, hydræmia, plethora, disturbances caused by the pregnant uterus, damming up of the circulation, the act of parturition itself, the diminished blood-pressure owing to the emptying of the uterus, prolonged labour, injury to the inner surface of the uterus, insufficient contraction, defective involution in childbed, the diminished flow or cessation of lochia, the suppression of milk secretion, death of the *fœtus in utero,* some idiosyncrasy of the individual. Such are the influences which have so much or so little effect in causing puerperal fever. But whether they are harmful or not, they can give no explanation of the striking difference in the mortality; all were the same in their incidence in the Second Division."

Along with the allegations regarding the causes of the higher mortality in the First Division in which I could find no satisfactory explanation, there were other circumstances connected with the First Division alone, for which no explanation whatever had ever been offered.

In the First Division all women in labour in whom the first stage was so tedious that it lasted 24 or 48 hours or longer sickened almost without exception, either during the process of parturition or within the first 24 or 36 hours after labour, and died of puerperal fever running a rapid course. Such lingering labour in the Second Division was devoid of danger, and no bad consequences were observed to follow.

As such a tedious course of the stage of dilatation as a rule occurs only in primiparæ, it was chiefly among primiparæ that the deaths occurred. "I often and often called the attention of my students to my conviction that some blooming young woman in exuberant good health with a tedious first stage of labour would, either actually during labour or immediately after the birth, become ill and die of puerperal fever marked by a rapid course. My prognosis was almost invariably correct; I did not then understand why it should be so, but I observed the sequence of events often enough; and the fact was all the more inexplicable that no such thing occurred in the Second Division."

This was true of the tedious first period, not of the expulsion stage, so there could be no question of traumatic causes.

But not only the mothers with prolonged first stage but also the new-born infants, without regard to sex, died of puerperal fever. The anatomical changes observed in post-mortem examination of the new-born were, apart from the genital sphere, identical with those seen in the cadavera of the victims of puerperal fever.

If it is one and the same disease from which lying-in women and their infants die, then the etiology of the disease in the new-born must be the equivalent of the etiology of the mother's malady. But a much larger number of the new-born died in the First than in the Second Division (Table XIV.).

The factors which produce the equivalent of child-bed fever in the new-born are at work before the completion of labour, or the disease is conveyed to the child after birth.

By a long process of elimination of the alleged factors in the production of child-bed fever in pregnant or puerperal women, which cannot possibly apply to the new-born, it is demonstrated that the ordinary conception of puerperal fever in the woman must be erroneous.

Street-births: Owing to the great expanse of the city of Vienna it often happened that women were overtaken with labour on the way to the hospital.

These women who had finished their labour on the way to the lying-in hospital—on the glacis, under archways, anywhere—reached the hospital with the new-born babes in their arms. These cases counted for charity, just the same as if the birth had occurred inside the hospital, and the new-born were admitted to the Foundling Hospital and maintained by the State. Many such cases occurred owing to attempts to evade "public instruction." The women were confined in the houses of midwives, and then came to the hospital alleging *Gassengeburt* (Street-birth). The authorities appear to have winked a good deal at these practices.

In the genuine cases of *Gassengeburt* the puerperium was usually free from illness of any kind, and this in spite of the unfavourable condition of the women during labour and immediately afterwards. They had to walk to the hospital in all states of the weather, often ill-clad and ill-shod. What was it that protected these women from the unknown injurious endemic influences at work in the First Clinic?

At the Second Clinic the health of the lying-in women after street-birth was just as favourable as in the First Clinic; that this did not attract much attention was simply because of the usually more favourable health conditions of the patients generally in the Second Division.

" This would be the place for demonstrating by statistics the smaller death-rate among the cases of *Gassengeburt* compared with the First Clinic as a whole."

" So long as I had the notes of the cases attended at the First Clinic at my disposal I did not feel the need of a table of statistics of street-births, for no one denied the facts, and consequently I delayed preparing statistical tables. Later when I had ceased to be assistant, they began to deny the facts, just as they afterwards refused to admit that there had ever been any difference in the mortality of the two Clinics, a difference which is undeniably established by my Table I."

When Skoda moved for the appointment of a committee of professors to inquire and prepare statistics, including a table to show the comparative results in cases of street-birth, Klein protested, and the committee, already nominated *auf höheren Befehl,* never met. Klein in fact obtained the interference of the Minister of Education to prevent investigation.

Another class of cases in which puerperal sickening was remarkably rare was that of *premature labours.* Women who were prematurely confined were exposed not only to the endemic influences of the First Division but to the additional danger resulting from the cause of the premature labour. Whence then the smaller incidence of abnormal puerperal processes among these cases in the First Division?

The explanation that the more premature the birth the less developed was that puerperal condition which predisposes to puerperal illness was not convincing, for the puerperal condition is not essential to the production of puerperal fever, and puerperal fever may originate during labour and even during pregnancy.

The better health conditions of puerperæ after premature labour in the Second Division was also in harmony with the better health condition of puerperæ after labour at full term in this Division.

Distribution of infected cases in the wards. The patients as a rule sickened in child-bed in an irregular manner: one woman would become the victim of puerperal fever while several women in the neighbouring beds, to right and left, remained well; but occasionally the patients in whole rows of beds in a ward sickened about the same time without a single normal puerpera between them.

How was this circumstance to be explained? In the Second Division patients who became ill were always scattered, never in rows? The explanation which appeared at the time sufficient was that puerperal fever is at times a contagious disease, and the occurrence of cases in rows (reihenweise) was declared to arise out of the spread of the contagion from bed to bed.

The authorities did not remain indifferent to the disquietening appearance of a higher mortality in the First Clinic compared with the Second, and various Commissions were from time to time appointed to inquire into the causes of the difference in mortality and to come to some conclusions as to whether a larger proportion of the lying-in women might or might not be saved in the First Clinic. In the hope of saving a larger number all puerperal fever cases were at once transferred to the General Hospital where they came under the treatment of different physicians, but they died there also with few exceptions.

By these Commissions, largely composed of non-medical dignitaries, sometimes the mortality was attributed to one of the endemic causes, sometimes to another, and sometimes to several combined, and certain measures were adopted accordingly without bringing the mortality in the First Division down to that of the Second.

Towards the end of 1846 a Commission reported that the prevalent puerperal illness was the result of *injuries to the genital organs* produced by the examinations made for purposes of instruction, although such examinations were also made by pupil-midwives in the course of their training. In order then to make intelligible the more frequent occurrence of puerperal illness in the First Clinic, blame was cast upon the students of medicine, especially the foreign students, for making examinations in a rougher manner than the midwives.

The practical result of this report was that the number of students was reduced from 42 to 20. Foreigners were almost completely excluded, and the number of examinations was reduced to a minimum.

The mortality then diminished to a marked extent in December, 1846, and during the first three months of 1847, but in spite of all precautions it rose in April, 1847, to over 18 per cent., and in May to over 12 per cent. From all which facts it became evident that the alleged cause was not the real cause of the mortality.

TABLE XV. shows a frightful mortality in 1846. If the month of December is excluded we reckon 443 deaths among just over 3,000 patients, which amounts to an average mortality of 14·5 per cent. for the year.

So the Reports of the Commissioners fatuously attributed the high mortality in the First Clinic "to epidemic causes with unusual characters," and they had no remedy to propose. It was the confusion of epidemic with endemic influences, owing to the incomprehensible contradictions observed, which delayed so long the discovery of the true causes of puerperal fever.

"I was convinced that the greater mortality in the First Clinic was due to some unknown endemic cause which

I had looked for in vain; I reflected on the import of the child-bed fever which attacked the new-born without distinction of sex, and I was misled through the observation of phenomena for which I could find no explanation. I had also to think of the deaths almost without exception in cases of protracted first stage in contrast to the immunity of the *Gassengeburten* (Street-births), and of women prematurely confined, which was in contradiction to my belief that the devastations of puerperal fever in the First Clinic must be attributed to endemic causes. Then there was to be considered the occurrence of cases of child-bed fever in rows in the First Clinic, the more favourable health conditions of the Second Clinic without any reason for believing that the Staff of the Second were more skilful or more careful than that of the First; and added to all was the contempt with which the resident staff of the First Clinic were treated by even their own domestic attendants; all this reduced me to such an unhappy frame of mind as to make my life far from enviable. Everywhere questions arose; everything remained without explanation: all was doubt and difficulty. Only the great number of the dead was an undoubted reality."

Some idea of the depression of spirits and perplexity produced by the failure of all his efforts to understand the nature of the disease, or to diminish its ravages, may be formed from the resort to the lateral instead of the dorsal position during labour. "Like a drowning man clutching at a straw, I gave up the dorsal position in labour, which was customary in the First Clinic, and introduced the lateral position, for no other reason than because it was that adopted in the Second Clinic. I did not believe that the dorsal position was so disadvantageous compared with the lateral position as to cause the higher mortality in the First Division, but the results were better in the Second Division, and part of the cause might be the lateral position."

Removal from Assistantship.

In October, 1846, when Semmelweis was fully occupied with the routine professional work of his office, and distracted and depressed in spirits by the prevalence of puerperal fever, he was suddenly deprived of his position as Ordinary Assistant by an unprecedented circumstance. The predecessor of Semmelweis, Dr. Breit, after his period of office had terminated, and his duties had been performed for several months by his successor, now obtained a two years' extension of his appointment. So Semmelweis had the humiliation and disappointment of returning to the position of provisional assistant for another two years. The degradation of Semmelweis was a scandalously arbitrary abuse of authority on the part of Professor Klein, from which it may fairly be concluded that the young Hungarian had exhibited disconcerting qualities as assistant by which he had forfeited the goodwill of his easy-going chief. The restless energy of Semmelweis in the First Clinic, his sympathy with suffering women, and his constant criticism of the old orthodox opinions on the causation of puerperal fever, must have all been the subject of conversation and strictures in the professional circles of the General Hospital, and it would be contrary to all experience if some backbiting, by repetition and exaggeration of utterances, did not reach the ears of the Director.

Whatever may have been the influences at work, it came to pass that the conventionally careful orthodox man-wife, Dr. Breit, once more assumed the duties of Ordinary Assistant in the First Division, and Semmelweis returned to his occupations as provisional assistant with the prospect of waiting for another two years in the inferior position.

The question then arose: How was he to occupy his time? Familiar with the history of the Vienna Medical School he naturally thought of the journey of Boër, and how his visit to the lying-in hospitals of Great Britain and Ireland had resulted in such advantages to obstetrical science and practice in Austria. Boër had introduced the methods of English midwifery practice into Vienna and obtained results hitherto unheard of, but Boër's theories of the causation of puerperal fever, though different, were no advance on the unsatisfying principles of former generations. Still, in Great Britain devastating outbreaks of puerperal fever seldom occurred in the lying-in hospitals. Was it possible then that by observing and studying British methods and opinions Semmelweis might obtain light upon the heart-breaking mystery of puerperal fever?

"In the winter of 1846–47 I employed my time in learning the English language with the object of spending the most of the time which I would have to wait until I could resume the post of assistant . . . at the great lying-in hospital of Dublin; but in the end of February, 1847, Dr. Breit was nominated professor of midwifery at the University of Tübingen, and so, changing my plan of a professional tour, I went in the company of two of my friends on a visit to Venice before taking over the duties of Assistant." The painful experience of the Vienna lying-in hospital had produced a depression of spirits and a feeling of discouragement, and a change of scene and occupation was required before the duties of ordinary assistant were resumed.

SECOND TERM AS ASSISTANT. THE DISCOVERY.

Death of Kolletschka.

"On the 20th of March of the same year (1847), a few hours after my return to Vienna, I resumed with renewed energies the duties of the post of assistant in the First Obstetric Clinic, but I was soon shocked by the sad news that Professor Kolletschka, whom I held in the highest honour, had died in the meantime." The following is the history of the case: Kolletschka, professor of medical jurisprudence, often performed post-mortem examinations in medico-legal cases with the assistance of his students. In the course of one such examination he received a punctured wound of the finger from the knife of one of the pupils. . . Professor Kolletschka thereupon became affected with lymphangitis, phlebitis in the same upper extremity, and he died from pleurisy pericarditis, peritonitis, and meningitis, and a few days before his death metastasis occurred in one of the eyes. . . . In the excited condition in which I then was, it rushed into my mind with irresistible clearness that the disease from which Kolletschka had died was identical with that from which I had seen so many hundreds of lying-in women die. The puerperæ also died from phlebitis, lymphangitis, peritonitis, pleuritis, meningitis, and in them also metastases sometimes occurred.

" Day and night the vision of Kolletschka's malady haunted me, and with ever increasing conviction I recognised the identity of the disease from which Kolletschka died with the malady which I had observed to carry off so many lying-in women."

The former conclusions with regard to the identity of the fatal disease of new-born infants with that child-bed fever so fatal to the mothers also recurred to the mind, and supported the conviction with regard to the malady of Kolletschka." The cause of the fatal illness of Kolletschka was well known: it was the wound produced by a dissecting scalpel which was foul with cadaveric particles. It was not the wound, but the wound rendered unclean by cadaveric material, which had produced the fatal result; and Kolletschka was far from being the first to die in this manner. If then it be granted that the disease by which Kolletschka lost his life and that from which so many lying-in women died, are identical, the cause of the disease in the lying-in women and that of Kolletschka must be the same. In the case of Kolletschka the cause of the disease was cadaveric material carried into the vascular system: I must therefore put this question to myself: Did then the individuals whom I have seen die from an identical disease also have cadaveric matter carried into the vascular system? To this question I must answer, Yes!"

Owing to the anatomical tendency of the Vienna School of Medicine, professors, assistants, and students had frequently occasion to come into contact with the cadaver. According to the usual method of washing the hands, merely with soap and water, the cadaveric particles adhering to the hands were never completely removed, a fact demonstrated by the cadaveric odour which the hands retained for a longer or shorter time after washing. In examinations of pregnant, parturient, and puerperal women, the hand made unclean by cadaveric material was brought into contact with the genitals, hence the possibility of resorption and by resorption the conveying of cadaveric matter into the vascular system of the patient. In this way was produced in the lying-in woman the same disease as that from which Kolletschka died.

If this theory that the cadaveric material adhering to the hand can produce the same disease as the cadaveric particles adhering to the scalpel be correct, then if the cadaveric material on the hands can be completely destroyed by chemical agencies, and the genitals of the woman in labour or in the lying-in state be brought into contact with the clean fingers only, and not simultaneously with cadaveric particles, then the disease can be prevented to the extent to which it originated by the presence of cadaveric material on the examining fingers.

Introduction of Antisepsis.

In order to destroy the cadaveric material adhering to the hands, I began about the middle of May, 1847, to employ *chlorina liquida* with which every student was required to wash his hands before making an examination. After a short time a solution of chlorinated lime was substituted because it was not so expensive. In the month of May, 1847, the mortality in the First Clinic still amounted to over 12 per cent., within the remaining seven months it was reduced in a very remarkable degree.

During these seven months, 1841, cases of labour were attended in the First Clinic and 56 women died, that is equal to a mortality of 3 per cent., whereas in the year 1846 before the introduction of chlorine disinfection out of 4,010 lying-in women in the First Division 459 died, that is a mortality of 11·4 per cent. In the Second Division in the year 1846 out of 3,754 patients 105 died, that is equal to 2·7 per cent.

In the year 1848 during the whole of which chlorine disinfection was diligently practised, 45 patients out of 3,556 died in the First Division, equal to a mortality of 1·27 per cent., and in the Second Division 43 patients out of 3,219 died, that is a mortality of 1·33 per cent. So for the first time in the history of the Vienna Lying-in Hospital the mortality in the First Division fell below that of the Second Division.

In this year, 1848, there were two months, March and August, in which not one single death occurred among the patients of the First Division.

The experience of every month went to support the belief that puerperal fever was nothing more or less than cadaveric blood-poisoning. A review of the history of the hospital, and its division into two Clinics, of the comparative practices of the two divisions and of the various teachers, and of the methods of instruction, all appeared to justify the conviction that the hitherto unknown endemic cause of the frightful devastation which puerperal fever had wrought in the First Clinic had at last been discovered; it was simply the cadaveric material adhering to the examining hands in the First Clinic. That the cause was cadaveric poison and that alone was further demonstrated by this fact, that with the exception of the introduction of chlorine disinfection no other change whatever had been made in the conditions prevailing in the First Clinic. The cause had been removed, the effect, that is the puerperal mortality, had disappeared.

The striking difference in the mortality in the two divisions was now also explained. The system of instruction for midwives was such that neither teachers nor pupils had so frequently occasion to manipulate the cadaver, and so render their hands unclean. Hence the comparative immunity of the Second Division from puerperal fever.

Discovery—Extension of Etiology.

The details of the chlorine disinfection were as follows: Every visitor to the labour ward (Kreissezimmer) who might have to make an examination must, on entering, wash his hands in a solution of chlorinated lime, and this chlorine disinfection once for all was considered sufficient for that visit. After making the first examination the student was required, before proceeding to another patient, to employ merely soap and water for the cleansing of his hands. This practice was based on the assumption that cadaveric particles adhering to the hands were the only cause of infection, and the cadaveric poison could be destroyed once for all. But a further extension of the doctrine of infection had soon to be made, and the practice of disinfection modified accordingly.

Incidence of the Medullary Cancer.

In October 1847 a woman was admitted to the Labour Ward and placed in Bed No. 1. This patient was discovered to be suffering from foul-smelling medullary cancer of the cervix uteri: and Bed No. 1 was that with which the daily visit of the staff and students always began. After examining this parturient patient we all washed our hands merely with soap and water: the consequence of these proceedings was that of twelve women confined at the same time eleven died from puerperal fever.

The explanation was that the putrid matter from the foul medullary cancer had not been destroyed by the washing with soap and water, and as the examinations proceeded the putrid infecting material was conveyed to the genitals of the other women who were in labour, and so the puerperal fever was spread.

From this experience the inference had to be drawn that not only cadaveric particles adhering to the hand, but that putrid matter derived from living organisms produces puerperal fever. Consequently it is necessary that the examining hands, not only after manipulating the cadaver, but even after examination of individuals from whom putrid matter might render foul the hands, must also be disinfected by the chlorine process before another patient is examined.

Taught by this sad experience we adopted measures of prevention, and we never again spread the childbed fever by conveying putrid matter by means of the examining fingers from one individual to another.

The carrier of the cadaveric particles, and the putrid matter derived from the living organism which might produce puerperal fever, was therefore the examining finger.

Incident of the Carious Knee-joint.

Another painful experience convinced us that the bearer of the decomposed organic matter which produces puerperal fever may be also the atmospheric air.

In November of this same year, 1847, a woman was admitted suffering from caries of the left knee-joint. Her genitals were perfectly sound, so that the examining finger remained for the other patients entirely devoid of danger. But the foul exhalations from the carious joint were so strong, that the air of the labour-room in which she was confined, was so loaded that all the patients in the same room became infected, and they nearly all died. The Report of the First Clinic shews that in November eleven patients died, and in the month of December eight deaths occurred, and they were for the most part caused by the foul exhalations from the woman with the carious joint.

The air of the labour-room, loaded with the putrid matter, found its way into the gaping genitals just at the completion of labour, and onward into the cavity of the uterus where the putrid matter was absorbed, and puerperal fever was the consequence. Henceforth patients presenting any similar conditions were isolated, and similar misfortunes were prevented.

We have here, for the first time, stated the theory of Semmelweis concerning infection by the atmosphere, and we find it frequently repeated and restated in the *Ætiologie*. Later in the history of puerperal fever we shall see authorities like Veit and v. Winckel and their early contemporaries, beginning to call in question the importance of the atmosphere as a factor in the production of puerperal fever. In this particular case of caries it is not difficult to imagine the shocking neglect, and the indifference to cleanliness, which permitted the nursing staff so to dress the sores as to cause the labour-room to be filled with a stench. The same carelessness would no doubt leave the dressings of the knee in such a state as to contaminate accidentally the hand employed to explore the genitals. In explanation of this indifference it should be always kept in mind that the whole lying-in hospital was constantly pervaded with offensive odours to which all were accustomed. There was the puerperal odour (*Puerperalgeruch*) and the smell of the *Abort* or closet, only one to a whole Division. We shall find later Arneth, who was the colleague of Semmelweis in the Second Division, exclaiming in admiration at the absence of evil smells in the London and Dublin lying-in hospitals.

The Discovery of the Causation of Puerperal Fever was now complete: the three sources of infection to which we shall have so many references throughout the *Ætiologie* and the *Briefe* were established facts in the mind of Semmelweis. Instead of the doubts and questionings of 1846, when all was uncertainty except the appalling number of the dead, to the mind constantly filled with the subject everything became clear, and events and phenomena took their places in an orderly system in establishing the truth of the Doctrine.

Explanation of Phenomena as Evidence.

Events in the history of the Lying-in Hospital, unintelligible hitherto, now poured forth evidence. The Lying-in Hospital was opened in August 1784, and Boër became professor of midwifery in 1789, after his return with obstetric *spolia opima* from Great Britain: he introduced the English methods of practice which he had learned, chiefly from Denman, founded on the principles of cleanliness and patience. From first to last, Boër's mortality from puerperal fever was comparatively insignificant. During the thirty-three years of Boër's incumbency until his dismissal in 1822, the patients amounted to 65,000, and of these 850 died, that is equal to an average annual mortality of 1·3 per cent.

In 1822 the last year of Boër's tenure of office, the mortality was 0·8 per cent. Boër had absolutely refused to teach midwife-pupils by practice upon the cadaver, and his want of subordination was made a pretext for his removal. His successor, Professor Klein, was more complaisant: he demonstrated on the cadaver, and during his first year of office, 1823, the mortality rose to 7·8 per cent., and since then it had never been eradicated: it usually remained high; sometimes it was appalling. The only difference between Boër the master and Klein the weak disciple was the introduction of cadaveric poison.

In 1846 the mortality was 8 per cent. In 1847, during which chlorine disinfection was introduced, the mortality was 3 per cent., and in the year 1848, during the whole of which the chlorine disinfection was diligently practised, the mortality sank to 1·28 per cent. Well might Semmelweis say that his Table XVII. (*Ætiologie*, p. 62), from which these figures are quoted, is "an irrefragable proof of my opinion that puerperal fever originates in the carrying over (Uebertragung) of decomposed animal-organic matter. At the time when the system of teaching restricted the opportunities of conveying infection the health of the patients was favourable; when the Vienna School adopted the anatomical basis of instruction the unfavourable health condition of the lying-in women began . . ."

A review of the past of the lying-in hospital also shewed that the less the amount of teaching carried on the more favourable were the health conditions of the women confined in it.

The *Division of the Lying-in Hospital into two Clinics* might be said to amount to an experimental proof by statistics of the Semmelweis Doctrine. The import of the vast difference between the mortality in the First and Second Divisions as shewn in Table I. now became clear. Broadly stated the mortality in the First Division was throughout three times that of the Second.

Then there were curious fine distinctions at times which added, if possible, to the completeness of the demonstration. "The variations in the mortality as they occurred in each of the Divisions could be attributed to the special occupations of the various members of the staff." When an assistant took special interest in pathological anatomy and made many post-mortem examinations, the mortality was high. Semmelweis recognised that he himself had been one of the greatest sinners. "Consequently must I here make my confession that God only knows the number of women whom I have consigned prematurely to the grave. I have occupied myself with the cadaver to an extent reached by few obstetricians . . . However painful and depressing the recognition may be, there is no advantage in concealment; if the misfortune is not to remain permanent the truth must be brought home to all concerned."

With all this dissecting and operating upon the cadaver it became clear to Semmelweis that the very conscientiousness of his own clinical work had been a source of danger to the patients, and then certain clinical experiences quite inexplicable hitherto took their place as evidence in support of the *Lehre*.

Evidence from Routine Clinical Work.

In the forenoon the professor accompanied by the students made a general visit to the lying-in wards, and every pregnant and parturient woman was examined by the students as part of the routine clinical instruction.

In the afternoon a similar visit was made by the assistant along with the students.

In the morning the assistant went round and examined every patient before the professorial visit in order to make his report to the professor.

As time permitted the students made examinations of special cases between the assistant's first round and the visit of the professor.

Consequently, when a patient with a tedious dilatation stage lasting a whole day or longer was so frequently examined with unclean hands she became infected, and such patients almost without exception died of puerperal fever.

After the introduction of chlorine disinfection and consequently of examination with clean hands, the deplorable mortality among these cases of tedious labour ceased, and delay in the first stage was no more dangerous in the First Clinic than it had always been in the Second Clinic.

In order to make intelligible the import of childbed fever in relation to the infection of the newborn, some of the matter which is contained in *Der Begriff,* which is treated of later, is stated here in anticipation (*Theilweise anticipirte*).

In childbed fever the first fact is a resorption of a putrid animal-organic material: in consequence of this resorption there follows a blood-dissolution (Blutentmischung). In cases of prolonged first stage the symptoms might appear during or immediately after labour. This might occur while the circulation of the mother and fœtus was still in organic connexion through the placenta, and consequently the fœtus became infected and was attacked by a disease identical with that of the mother. . . . The healthy mother could not convey childbed fever to the newborn. This is the explanation of the fact that the newborn never died of puerperal fever while the mother remained well.

We had now also an explanation of former observations with regard to " street-births." It was now easy to understand why these patients so seldom sickened after admission to the First Clinic: these patients were no longer objects of interest for clinical demonstration, and therefore they were seldom examined after admission, and consequently they escaped contact with hands to which cadaveric material adhered.

Similarly, with regard to premature labour. There was seldom occasion for frequent and exact examination. These labours were completed rapidly without further complication, and being rarely of much obstetric interest they escaped examination and consequent infection.

Also the *reihenweise Erkranken* or sickening in rows found an easy explanation. Owing to the large number of patients in the First Clinic there were often several in the labour-room at the same time. Twice daily at the least during the professor's visit, and again during the assistant's visit in the afternoon, all these women were examined for the sake of practice by the students in the order in which they lay in bed. They were thus infected in sequence by the unwashed hands of those who came from the dissecting-room. After labour the patients were transferred to the lying-in wards and placed in bed in the same order in which they had been confined. Hence it was that puerperal fever attacked the patients *reihenweise* just as they had been infected by frequent examinations during labour.

After the introduction of chlorine disinfection the "sickening in rows" ceased to occur.

We have seen that as the result of the report of a Commission the number of examinations of patients in labour was much reduced and foreign students were excluded. Following the adoption of these regulations the mortality fell considerably in the end of 1846, but rose again to an alarming extent in April and May 1847. The explanation of these variations is a remarkable testimony to the truth of the doctrine of cadaveric poisoning.

"Owing to the circumstances in which I was placed as aspirant for the post of assistant in the First Obstetric Clinic, later as provisional assistant, and finally as actual assistant, it was not possible for me to study gynæcology in the gynæcological department of the General Hospital. As substitute for this . . . from the year 1844 until I returned to Pesth in 1850 I was accustomed almost daily before the morning visit of the professor to examine all the female bodies in the dead-house of the General Hospital with the object of studying gynæcology. Professor Rokitansky, whose friendship I could boast of (ich mich rühmen konnte) . . . had the kindness to permit me to make post-mortem examinations of all the female bodies which were not required by himself for section, and consequently I was able to verify the results of my examinations by dissection.

For various reasons the Assistant in the First Clinic made very few visits to the dead-house in December 1846, and during January, February, and March 1847, and the students, reduced in number to 18, followed his example, consequently the occasions for fouling the hands with cadaveric material were very much reduced in pregnancy, and clinical examinations had been reduced to the minimum. This explains the diminution in the mortality in the First Clinic during the months indicated.

On the 20th of March, 1847, I assumed the duties of actual assistant in the First Clinic for the second time. I had then to carry on my gynæcological studies in the dead-house in the early morning hours before visiting the labour-room. In this room it was my duty to examine every patient in labour so as to be able to report on every case to the professor during his morning visit. Consequently I was constantly bringing my fingers, foul with cadaveric particles, into contact with the genitals of parturient women. The result was that in April 57 patients died out of 312 confined, equal to 18¼ per cent., and in May 36 died out of 294 confined, that is 12¼ per cent.

After the employment of chlorine disinfection with its happy results, the number of students permitted to make examinations was increased, and examining during labour went on as in former times just as required for instruction, but the First Clinic had lost its painful distinction for high mortality as compared with the Second Clinic.

The Commissioners of 1846 had declared that the foreign students were more dangerous to the patients because they made examinations in a rougher manner, but that was not the real reason. It was this: The foreigners come to Vienna to complete, in a few months, the medical studies which they have begun at some other university. They attend the pathological and medicolegal autopsies at the General Hospital. They take courses of pathological anatomy, of surgical, obstetrical and ophthalmological operations on the cadaver, and they attend the clinical work in medicine and surgery: in a word they do all they can in the time at their disposal, and they consequently find frequent occasions for rendering their hands unclean with putrid animal-organic material, and when they are practising midwifery as well, simultaneously, it is easy to understand how they endanger the lives of lying-in women.

"On that account we have no more right to reproach them than to blame myself and others who, when we knew no better than examine parturient women with hands smelling of the dissecting-room, caused so many deaths."

With the object of supporting my opinions by means of direct investigation and proof, I thought it advisable and necessary to make some experiments on animals. These experiments were conducted upon rabbits with the help of my friend Dr. Lautner, assistant to Professor Rokitansky.

IV.

SPREAD OF THE DOCTRINE DURING THE VIENNA PERIOD.

Now, in the autumn of 1847, was the Discovery of Semmelweis complete, and the Doctrine firmly established in his own mind. It amounted to this: that puerperal fever was caused by a decomposed animal organic matter conveyed by contact to the pregnant, parturient or puerperal woman without regard to its origin, whether from the cadaver or from a living person affected with a disease which produced a decomposed animal organic matter.

We shall find that with years of experience Semmelweis found many varieties of illustration, but he never modified his Doctrine in any manner or degree, whatever opponents in later years may have alleged as an excuse for carelessness in the reading of his works, or for their own hostility and the eager levity with which they misrepresented or misunderstood him.

For Semmelweis it was the "eternally true" doctrine of the etiology of puerperal fever. The explanation of why and how decomposed animal matter infects the puerpera was to come with later developments of biological science, but the etiology of Semmelweis stands to-day, without essential modifications, as it was announced sixty years ago.

It may be assumed that while the etiology was becoming evolved and taking shape in the mind of Semmelweis, his intimate friends Hebra, and Skoda, and Rokitansky were kept informed about every episode bearing on the question which occurred in the First Obstetric Clinic; and no doubt the large professional circle of the General Hospital became acquainted with the new doctrine in its various stages of evolution, incomplete and complete, and received the tidings with cordial satisfaction, or the reverse, according to their interests and sympathies.

Professor Klein would have nothing to do with the new-fangled notions of his Assistant, but he did not at first actively oppose Semmelweis or interfere in any way with his professional work in the Clinic. Klein had ceased to take much interest even in the teaching of his subjects; he merely delivered his lectures, and left the management of his department very much in the hands of Semmelweis. All which was a distinct advantage towards the experimental establishment and the progress of the *Lehre*.

The most ardent supporters of the Semmelweis Doctrine were the three professors who had befriended him from the first, with Dr. Haller, senior physician at the General Hospital, the group of men in fact who had drawn the attention of the world to the Vienna School of Medicine, and attracted to their class-rooms in the General Hospital students and young graduates from all parts of Europe. These men of light and leading "doubted not for a moment that the theory would prove itself right," as Skoda said in an address two years later. Probably as a result of mere temperament, and possibly also influenced in some measure by the recollection of the ill-usage to which he had himself been subjected as the pioneer of auscultation and percussion, Skoda became the most strenuous and outspoken supporter of Semmelweis, and he may have gone just a little too far in his advocacy for the best interests of his protégé. In any case, influential as he was in some quarters, he could not at first obtain the appointment of a Commission of Inquiry into the importance of the discovery, and a bitter feud arose between him and Klein over the subject. One result of this, as far as Semmelweis was concerned, was that Klein, instead of remaining a tolerant observer or passive resister, became an active and dangerous enemy.

With Klein hostile, we may be sure the usual circle of sycophants would stir up animosity, and encourage students and others to obstruct the exact and thorough application of the method of prophylaxis introduced by Semmelweis in May, 1847. There were occasional difficulties at the Clinic arising from this cause, which broke the even tenour of the smooth and conventional routine. For example some of the students used to make game of the Assistant's 'fad,' and one in particular, who was engaged in dissection, disregarded the regulations about disinfection before making examinations in the labour-room. Suddenly the mortality rose in September, 1847, to 5·25 per cent. Careful inquiry led to the identification of the culprit. The levity and want of conscience which had sacrificed the lives of several poor women, was just the kind of thing to rouse the anger and contempt of an earnest and kindly man. Semmelweis said to the young criminal some "easy things to understand": his anger was frightful; and he employed terms of such unmeasured reprobation and warning that playing tricks on the eccentric assistant ceased henceforth to be a source of amusement to even the most frivolous student.

Semmelweis referred to some such incidents, in after years, as probably explaining the failure of chlorine disinfection at the lying-in hospitals where the Directors had no faith in the efficacy of the measures which they professed to employ.

In the autumn of 1847 the Obstetric Clinic was visited by Kussmaul and a companion from Baden. They had both been assistants to the now aged and honoured Naegele, of Heidelberg, and on that ground alone they were welcomed by Semmelweis as personal friends. He obtained for them the privilege of working at the Obstetric Clinics for six weeks.

Kussmaul pronounced the operation course of midwifery, conducted by Semmelweis, as simply admirable; and he appears to have been much impressed with what he witnessed of the work of the Vienna Lying-in Hospital. He referred to Semmelweis in the most friendly terms many years afterwards in his *Jugenderinnerungen eines alten Arztes* (Youthful memories of an old physician).

According to Kussmaul's recollection, Semmelweis was, in 1847, "more than the medium height, broad and strongly built, his face round with somewhat prominent cheek-bones; his forehead was high and his hair was rather thin for his years; he had remarkably fleshy and dextrous hands. He was of a lively temperament and had great capacity and willingness for work. He had a warm and kindly heart, and was conscientious to an extreme degree." Semmelweis was at the time of Kussmaul's visit full of his great and beneficent discovery, and it was the constant subject of conversation between him and his visitors. On Kussmaul and his friend Profesor Klein made the impression that he was a very ordinary sort of practitioner, and that he had ceased to take much interest in his work, either as professor or clinician.

Kussmaul and his friend were still frequenting the Clinic when the incidents of the medullary cancer of the uterus, and the caries of the knee-joint occurred. Semmelweis, he said, at once recognised their bearing on the extension of his doctrine of cadaveric poison; and the practical conclusions to be derived and applied: not only the cadaveric particles adhering to the fingers produce puerperal fever, but the malady may be also produced by any putrid material derived from a living organism. It is therefore necessary to apply the chlorine disinfection to the hands after every examination when putrid material is present: the putrid material must also be chemically destroyed after examination with the same scrupulous exactitude as the cadaveric poison is destroyed before examination.

Hebra's First Article.

To Hebra, who was at that time editor of the *Journal of the Medical Society of Vienna,* the Doctrine appeared to be now sufficiently matured for public notice. He like all the young and active supporters of Semmelweis, was disgusted with the unworthy conduct of Klein, and he resolved to carry matters beyond the stage of mere gossiping discussion among professional coteries in the General Hospital and elsewhere. So Hebra wrote an article which appeared in December, 1847, in the *Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien.*

That article is so important historically that it is here given in full.

EXPERIENCE OF THE HIGHEST IMPORTANCE CONCERNING THE ETIOLOGY OF EPIDEMIC PUERPERAL FEVER AT THE LYING-IN HOSPITAL.

> " The Editor of this Journal feels it is his duty to communicate to the medical profession, in view of the prevalence of puerperal fever in all lying-in hospitals, the following observations made by Dr. Semmelweis, Assistant in the First Obstetric Clinic in the General Hospital of this city.
>
> Dr. Semmelweis, who has been already for five years at the hospital, has become thoroughly instructed at the dissecting table as well as at the beds of the patients in the various branches of the healing art. For the last two years he has devoted special attention to the subject of midwifery, and has undertaken the task of inquiring into the causes which lie at the basis of the prevailing epidemic puerperal processes. On this subject nothing has been left untested, and everything which could exercise an injurious influence has been carefully removed.
>
> By daily visits to an institution of pathology and anatomy Dr. Semmelweis had learnt what were the injurious influences which were produced by filthy and putrid fluids upon even unwounded portions of the body of individuals engaged in post-mortem examinations. These observations aroused in him the thought, that perhaps in lying-in hospitals the pregnant and parturient patients might be inoculated by the accoucheur himself, and that puerperal fever

was in most cases nothing else than cadaveric infection.

In order to test this opinion, it was laid down as a rule in the First Obstetric Clinic that every one, before making an examination of a pregnant woman, must first wash his hands in an aqueous solution of chloride of lime (Chloratis calcis unc. 1, Aqua fontana lib. duas). The result was surprisingly favourable; for during the months of April and May, when this rule had not yet come into force out of 100 cases of labour there were 18 deaths, the number of deaths in the following months up to November 26, only amounted to 47 out of 1,547 cases, that is to say the mortality amounted to 2·45 per cent.

From this circumstance the problem is perhaps solved, why in schools for midwives the proportion of the prevalent mortality is so favourable in comparison with that of the institutions devoted to the training of students of medicine. An exception is the Maternité of Paris where, as is well known, post-mortem examinations are undertaken by the pupil-midwives.

Three distinct facts of experience may perhaps still further confirm the conviction just expressed and even extend still further its scope. Dr. Semmelweis believes that he can prove that:

1. Owing to careless washing some student engaged in dissection caused the loss of several patients in the month of September:

2. In the month of October, owing to frequent examinations of a patient in labour, who suffered from a foul-smelling medullary sarcoma of the uterus, when washing was not practised. Also finally:

3. Owing to a filthy discharge from an ulcer of the leg in one of the patients, several women who were confined at the same time were infected.

Thus, therefore, *the conveyance of a foul exudation from a living organism may be one cause which produces the puerperal process.*

In publishing these experiences we invite the Directors of all lying-in institutions, some of whom Dr. Semmelweis has already made acquainted with these most important observations, to contribute the results of their investigations either to support or refute them.''

There are some inaccuracies in this article which an obstetrician would not have introduced, but they are not important, and they go to prove that Hebra himself wrote the article without the inspiration and supervision of Semmelweis. This point is therefore brought clearly out at this time, December 1847, in a widely-read medical journal that: *Puerperal fever is in most cases a cadaveric infection, but it is sometimes an infection by means of putrid exudation or discharge from a living organism.*

We shall see in the prolonged and acrimonious controversy, which followed even the publication of *Die Ætiologie,* a damnable iteration of the statement that the *Lehre* of Semmelweis was narrow and one-sided.

The constant reference to cadaveric poison, as by far the most important cause of puerperal fever, may surprise the English reader, and it requires a word of explanation. The practice of making post-mortem examinations was universal in Austria at that time and for long afterwards. From early in the nineteenth century all medical practitioners in Lower Austria were required by a ministerial order, which to them had the force of law, to make as many autopsies as they could, and to send interesting specimens to the General Hospital of Vienna for educational purposes. In private practice even it came to this: So many deaths, as many post-mortem examinations and dissections. Medical practitioners hurried from these autopsies, after perfunctory hand-washing, to surgical operations and midwifery cases.

It was required also that post-mortem examinations should be made upon the bodies of all patients who died in the hospitals. The professors and assistants performed these sections, and in the case of teachers of midwifery, they were ordered to use the bodies of the mothers and the dead infants for purposes of instruction instead of the phantom. It was this dangerous, senseless, and disgusting practice which Boër refused to comply with, and this refusal was made the pretext for depriving him of his position as professor of midwifery and director of the lying-in hospital. Klein, his successor, obeyed, as we have seen, and gave "epidemic" puerperal fever such an established position in the hospital that it could never be displaced.

It was because this extreme practice of making post-mortem examinations was not generally known in Europe, that the ready objection to the Semmelweis doctrine was raised in many quarters; cadaveric poison could not be the cause of puerperal fever, because their students did not dissect or make post-mortem examinations. Still the practice of post-mortem examination by the professors of midwifery and their assistants was almost universal in the lying-in hospitals of Europe, with the exception of Great Britain and Ireland.

Spread of the Doctrine by Correspondence.

Whatever may be pleaded in extenuation of the silence of Semmelweis, there can be no question that the time had now arrived when he himself should have enforced the announcement made on his behalf by his friend Hebra. The columns of the Austrian, Hungarian, and German medical journals were available to him for publication, and it was now, and not ten years later, that he was in duty bound to seize the opportunity of proclaiming his Doctrine. But he remained practically silent, and he and a group of young friends, chiefly foreign graduates who had been attracted to Vienna by the opportunities of clinical study, resorted to the comparatively futile methods of spoken word and private letter in order to spread the doctrine.

Among these visitors were Dr. F. H. C. Routh, of London, Dr. Stendrichs of Amsterdam, Kussmaul of Heidelberg, a former assistant of the now venerable

Naegele, Schwarz the young friend of Michaelis of Kiel, and Wieger of Strassburg.

So these young graduates, who had learned the Semmelweis Doctrine at first hand, and enthusiastically accepted it as a new evangel, divided Europe among them, and each undertook to write to his former professor or to some influential obstetrician of his own country.

Skoda also lent his great influence at Prague and Würzburg, but he thus unintentionally opened the floodgates of hostile controversy, as will appear in the sequel.

After the despatch of the letters we can imagine how the coterie of young *Fachgenossen* would meet in the hospital, in the Bier-Halle, or in the private lodging in the Josephstadt, and eagerly speculate on the reception their letters were receiving at their destination, and more or less anxiously anticipating the purport of the answers. The young men all thoroughly believed in Semmelweis, and were anxious to be helpful in spreading the truth. This fact alone affords strong testimony to the attractive personality of Semmelweis, and the impressiveness of his teaching.

THE REVOLUTION.

But there were other matters of the gravest importance filling all men's minds in those early days of 1848. The amazing portent of a reforming Pope of Rome had appeared in 1847, and Austria, with insignificant exceptions was devoted to Rome.

Probably owing to mistaken interpretation of papal action, a movement in favour of political liberty sprang up, and disturbances of the most serious character occurred in Milan and Venice, in the whole of Austrian Italy, in the first days of January, 1848. The political storm, about to burst upon Vienna, was to have a disastrous influence upon the career of Semmelweis. Generally speaking, we read very little in the history of medicine of the influence of political events, in any epoch, upon the advancement or arrest of the progress of medical science; and in this particular instance, in spite of the serious consequences of the revolution upon the whole subsequent career of Semmelweis, we note with astonishment an almost universal silence on the subject among his biographers.

Carlyle, writing about the Revolution of 1848, says: "Closely following the outbreak in Paris all Europe exploded, boundless, uncontrollable, and we had the year 1848 one of the most singular, disastrous, amazing and on the whole humiliating, years the European world ever saw. . . . The kings all made haste to go. . . . Not one of them turned round and stood upon his kingship as upon a right he could afford to die for. . . . Such was the history from Baltic to Mediterranean, in Italy, France, Prussia, Austria, from end to end of Europe in those March days of 1848. . . .

"And so in city after city street-barricades are piled, and truculent more or less murderous insurrection begins; populace after populace rises; king after king capitulates or absconds; and from end to end of Europe Democracy has blazed up explosive, much higher, more irresistible and less resisted than ever before. . . .

"The kind of persons who excite or give signal in such revolutions—students, young men of letters, advocates, editors, hot inexperienced enthusiasts, or fierce and justly bankrupt desperadoes—acting everywhere on the discontent of the millions and blowing it into flame."

Such states of agitation and excitement cannot last for long, but in Vienna, where the explosion occurred in March, the unrest continued for much longer than in the western and northern parts of Europe, largely owing to Race complications.

Early in March the Diet of Lower Austria had addressed a petition to the Emperor Ferdinand asking for certain reforms. On the advice of Metternich, the real autocrat, the petition was rejected, and at once the troubles began. There was a rising of students who broke into the Chamber and sacked it, and they then came into collision with the soldiers; forthwith the bloodshed began. It was immediately after these incidents that the "Academic Legion" was formed, consisting of professors and students, medical practitioners and lawyers. Semmelweis and other assistants in the various departments of the General Hospital enrolled themselves in this revolutionary force, and among these Semmelweis was considered one of the most active and enthusiastic champions of freedom. He was one of the contingent of Viennese enthusiasts who went out to meet the Hungarians who under the leadership of Kossuth were advancing upon Vienna to aid the revolution in Lower Austria, but had to retire in the face of a stronger force of Imperial troops.

Semmelweis has been described as sticking close to his professional duties in spite of all distractions, but we may take the liberty of assuming that he found in the early crisis little time for post-mortem examinations, and he would not be much troubled with medical students bringing cadaveric poison to the labour-room of the First Clinic. It was in this month of March, 1848, that for the first time in the history of the Lying-in Hospital, there was not a single death from puerperal fever, and no patient even sickened of the malady. Semmelweis refers evidently with much satisfaction to the record of March, but he does not claim in support of his doctrine, as he might well have done, that the patients were left almost entirely to chance and the care of the head-midwife.

Although Metternich had taken refuge in England, the reactionary bureaucracy remained at the head of affairs, and although they appeared to make generous concessions in Vienna, as they had done in Milan and Venice, they were only waiting their time. The troubles lasted with slight intermissions for the whole year.

Hebra, who had also joined in the revolution, said that Semmelweis came to attend Frau Hebra in her confinement wearing the uniform of the "Legion." We can only try to imagine the feelings of an old conservative *Höfling* like Professor Klein, when he first met his assistant in the Clinic dressed in the stylish uniform of the revolutionary "Legion," the broad hat with waving plume in one hand, and some implement in the other more relevant to the professional situation than the sword.

The Emperor and his family finally fled from Vienna in October, but it was not until after deplorable scenes of bloodshed, including assassinations, that Prince Windischgrätz dominated the city and order was restored. The Emperor abdicated in December in favour of his nephew Francis Joseph, and the revolutionary movement in Vienna came to an end. But its influence upon the fortunes of many participators was only to appear later, and especially was this true of the career of Semmelweis.

The present venerable Emperor was then a young man of eighteen; the reactionary bureaucracy dominated the situation in Vienna, Prague, Buda-Pesth, Milan and Venice, and they made their power felt. In Buda-Pesth the Revolution ruined the Semmelweis family.

When political affairs had been arranged, and routine medical work in Vienna had been resumed, a movement for medical reform acquired such importance that it had to be recognised by the new Minister of Public Instruction. This movement had far from a beneficial influence upon the professional career of Semmelweis.

Several of the arm-chair professors of the Medical Faculty had to be pensioned off, but Klein had such influence with certain official persons that he was permitted to retain his position.

The control of medical education was transferred to a Board of Studies consisting of the Faculty of Medicine, and it soon became evident that the Board was composed of two factions, the reformers and reactionaries, between whom a bitter feud soon broke out. Unfortunately for Semmelweis his personal friends and supporters were all reformers, his chief, Professor Klein, was an influential reactionary. Semmelweis could naturally take no part in the disputes of the professors, but he became a passive victim to the hostility of the more powerful faction towards his friends.

Spread of Doctrine by Correspondence Resumed.

The letters sent by the friends of Semmelweis to the professors of midwifery in various universities, clinics, and schools for midwifery in the Continent of Europe and Great Britain, received very little response, and the article by Hebra appeared to have attracted no attention from those official teachers whom it ought to have compelled into immediate action. Meanwhile the foreign visitors had all left Vienna to avoid the political revolution.

Simpson.

Among the letters sent to professors of midwifery was one written to Simpson of Edinburgh by Arneth, assistant in the Second Clinic, who knew English better than Semmelweis (der Englischen Sprache mehr mächtig als ich). Simpson was then engrossed in his experiments with chloroform as an anæsthetic, but he replied by return of post to Arneth's letter:—

> "This letter," says Semmelweis, "was filled with abuse (Schmähungen); Simpson said that without the letter he knew in what a lamentable condition midwifery in Germany, and especially in Vienna, still remained: he knew for certain that the cause of the high mortality lay only in the unbounded carelessness with which patients were treated; as, for example, when they put a healthy lying-in woman into the same bed in which another patient had just died, without changing the bedclothes and linen."
>
> Our letter also, he said, proved that to us English obstetric literature was quite unknown, otherwise we would have been aware that Englishmen had for a long time held that puerperal fever was a contagious disease, and they employed chlorine disinfection for its prevention. "This letter," remarks Semmelweis in his usual simple fashion, "did not make us feel disposed to continue the correspondence with Simpson!" It is obvious that Simpson (*in Folge*

> *einer Uebereilung*) missed the point of Arneth's communication, in considering the opinion of Semmelweis on the cause of puerperal fever as identical with that of the medical profession in England. Perhaps no incident in Simpson's life so clearly brings out the fair-mindedness of the man and his conscientiousness in seeking scientific truth without regard to mere personal considerations. As soon as he received a clear exposition of the Semmelweis doctrine from Arneth, who visited Edinburgh in the year 1851, he seized the point of difference, and frankly acknowledged his mistake and tried to make reparation.
>
> But after all Simpson's reproaches did not amount to *Schmähungen*. He merely stated what was true, somewhat abruptly perhaps. The lying-in hospitals of Austria, Germany and France were at that time atrociously neglected compared with those in England; and nowhere in Europe except Holland and Scandinavia had the staffs the slightest notion of the value of cleanliness. When Arneth visited the London lying-in hospitals, what appeared to impress him most was this cleanliness and the absence of offensive odour in the wards!

Simpson was mistaken concerning the identity of the Semmelweis doctrine with the English theory of contagion, but there can be no doubt that it was the preparation of a mental receptivity and capacity for appreciation which the doctrine of contagiosity, prevalent in the United Kingdom provided, that made the Semmelweis doctrine receive such a prompt and cordial welcome in London, Dublin and Edinburgh, much to the honour of our leading obstetricians of the time. We shall have to deplore a retrograde movement in England under French influence a generation later, but since the time of Simpson there has always been, especially in Scotland, a strong phalanx of advanced opinion which has been proved in time to have been founded on the truth, as it is now universally recognised and accepted.

Routh.

Semmelweis, when *Die Ætiologie* was written, saw these incidents in a different light:—

" That Simpson in his haste must have mistaken my opinion on the origin of childbed fever as identical with the opinion of the medical profession in England, is evident from a correspondence which I had with Dr. F. H. C. Routh, of London." . . . .

" Dr. Routh visited as a student the First Obstetric Clinic in Vienna when I was assistant, and what he witnessed convinced him of the truth of my *Lehre*. He returned to his native country with the resolution to spread the knowledge of my doctrine there.

" I received the first letter from him dated London, January 23, 1849."

English medical men were probably not so conversant with the German language then as they are nowadays, and Routh wrote in Latin, which was still, in spite of the secular agitation against it, the formal official language of the educated Magyar. Routh's letter was as follows:—

"Comitiis in ultimis septimanis Novembris (1848) convocatis, illic discursus, in quo tuam inventionem enunciavi, reddens tibi, ut voluit justitia, maximam gloriam, praelectus fuit. Enim vero possum dicere, totum discursum optime exceptum fuisse, et multi inter socios doctissimos attestaverunt argumentum convincens fuisse. Inter hos precipue Webster, Copland, et Murphy, viri et doctores clarissimi, optime locuti sunt. In Lancetto Novembris, 1848, possis omnia de hac controversia contingentia legere. . . . .

. . . . Febris ne puerperalis rarior est quam antea? Si morbus sic periculosus in cubilibus obstetriciis non adsit ut ante, certe effectus magni momenti denuo firmatus. In Praga quoque, ubi febris puerperalis tum frequenter obvenire solebat, eisdem causis consecuta fuit ingenerari!

Dr. Routh was the first Englishman to proclaim the Semmelweis doctrine in England. He read a paper at a meeting of the Medico-Chirurgical Society entitled: "On the Causes of the Endemic Puerperal Fever of Vienna," and it was published in the *Medico-Chirurgical Transactions*, Vol. xxxii (1849).

In this paper Dr. Routh laid too much stress upon cadaveric poison, as perhaps was natural under the circumstances.

After the reading of Routh's paper, Dr. Murphy mentioned the case of a German student who was constantly going to post-mortem examinations. Puerperal fever seemed to attend him wherever he went; but on giving up his pursuit after dead bodies the fever subsided.

Dr. Copland said that the facts stated in the paper were so convincing that he could scarcely doubt their accuracy. The mode of infection mentioned by the author was, however, only one of the modes in which puerperal fever was propagated. It was known that the disease might be communicated also by the hands of the accoucheur who had attended a case of the disease. . . . He thought that something was due to the frequency with which examinations during labour were made. . . .

Mr. Moore mentioned that the number of post-mortem examinations at the Vienna Hospital was remarkable. He had seen as many as fifteen bodies lying for examination in a morning. The students and professors had their hands immersed in these for hours together. . . . (*Lancet*, Dec. 9, 1848).

Michaelis.

The second answer to Semmelweis came through Schwarz from Professor Michaelis of Kiel, known to English readers chiefly by his work on the "Obliquely Contracted Pelvis," quoted by all our modern makers of text-books on midwifery. Michaelis wrote: "When I received your letter I was again in the greatest distress. Our institution had been closed on account of puerperal fever from the 1st of July to the 1st of November. The first three patients then admitted sickened, one of them died, and the other two were just saved. . . . Your communication gave me some encouragement for the first time. . . . I at once introduced your method of chlorine disinfection into our institution. . . . Our hands used to smell all day long of the dissected cadaver in spite of repeated washing, but the chlorine disinfection has put an end to that. Since the introduction of your method not a single case of labour, attended either by myself or my pupils, has shown the slightest degree (gelindeste Grad) of fever with the exception of one in February. . . . I therefore thank you for your communication with all my heart; you have perhaps saved our institution from destruction. . . . I beg of you to greet Dr. Semmelweis on my behalf, and to offer him my thanks: he has perhaps made a great discovery. . . . You are aware that puerperal fever broke out here for the first time in 1834, and that was about the time when students were required to make regular examinations of the patients. This circumstance may also have relation to the etiology. . . ."

Michaelis, after his experience of prophylaxis increased, began to brood over some of the deaths from puerperal fever which had occurred under his care, and he met with a tragic death at his own hands. "I have given an account of the painful incident," says Semmelweis, "in order to raise a monument to his conscientiousness."

Tilanus.

In an entirely friendly spirit is conceived the reply received from Tilanus of Amsterdam in 1848. The letter is interesting as throwing light on methods of practice founded on theoretical opinions now only known to the history of obstetrics. Tilanus does not yet see sufficient reason for entirely giving up the principles which have guided him in conducting a lying-in institution for twenty years. He firmly believes in the contagiousness of puerperal fever, and in the varying degree to which patients are susceptible. There is much in the epidemic atmospheric influences in which the *constitutio annua* in winter and spring is an important factor. "We often had the experience of the spread of an epidemic in the hospital by the admission of a patient already infected, and the conveyance of the contagion to other women owing to the atmosphere in which they were thus placed."

Tilanus from his own experience entirely agrees with Semmelweis as to the importance of cadaveric poison in producing puerperal fever, and he now employs rigorous measures of preventing infection from this cause. That the mischief may be diminished by means of chlorine disinfection he quite agrees, but that it can be completely rooted out by this process he cannot yet believe.

"I close with the wish that your efforts in the cause of humanity may deal a powerful blow to the ruinous disbelief in the contagious nature of this disease and of the injurious effects of cadaveric poison. When a man otherwise so able as Kiwisch von Rotterau declares that he frequently attends parturient and puerperal women immediately after making post-mortem examinations, it certainly sounds horrible, and must at the same time give an example of rashness and carelessness to the uninstructed."

We observe in the opinions of Tilanus a detachment from the strict epidemic theory then universally prevalent in Germany, and a sympathy with the British theory of contagion. The liberal and broad-minded character of the Amsterdam professor is shown by his sympathy with Semmelweis and partial acceptance of his doctrine.

Hebra's Second Article.

The appeal to the Directors of Lying-in Hospitals, whether they were Clinics for students of medicine or schools for midwives, contained in Hebra's article, published in December, 1847, met with little or no response. So after the first excitement of the revolution had passed, Hebra, who had also been a soldier of the Academic Legion, published a second article as a contribution to the propaganda for the spread of the principles and practice of Semmelweis. This article was also published in the Transactions of the Medical Society of Vienna.

The second article was as follows:—

CONTINUATION OF THE EXPERIENCES CONCERNING THE ETIOLOGY OF EPIDEMIC PUERPERAL FEVER IN THE LYING-IN HOSPITAL.

"In the December number of this Journal in 1847 there was published the highly important experience of Dr. Semmelweis, Assistant in the first Obstetric Clinic, with regard to the causation of the epidemic puerperal fever which occurs in lying-in institutions.

This experience consists (as the readers of this Journal will remember) in this, that lying-in women become ill especially when they have been examined (*touchirt*) by medical men who have had their hands rendered unclean by examinations of dead bodies, and have only washed them in the ordinary way, while no cases of illness, or very few, have occurred when the examining hands had first been washed in a watery solution of chloride of lime.

This highly important discovery, which is worthy of a place beside that of Jenner's small-pox vaccination, has not only received complete confirmation in our lying-in hospital, but assenting voices have been raised in distant foreign lands expressing belief in the correctness of the theory of Semmelweis. Among the letters received are those from Michaelis of Kiel and Tilanus of Amsterdam, from which especially we select corroborative testimony. Still in order to obtain for this discovery its full influence we would, in the most friendly manner, request all the directors of lying-in hospitals to set investigations on foot and to send the results obtained to the Editor of this Journal whether they support or refute the theory."

Hebra was the first to compare the beneficent discovery of Semmelweis with that of Jenner. Semmelweis accepted the comparison as just and appropriate, and introduced it repeatedly in later years when defending his *Lehre*. There was no exaggeration in favour of Semmelweis in the comparison: Hebra's expression has been amply justified since it was first employed.

Haller.

While all medical circles were discussing the value of the Semmelweis discovery, and taking part indirectly in the feud among the professors of the Faculty of Medicine, Dr. Karl Haller brought the subject before the Medical Society in February, 1849. He was preparing the annual report of the hospital for 1848, and had been deeply impressed with the statistics of the Lying-in Department. Haller had the record of the Lying-in Hospital for the last twelve years, and he stated that in the last year, as compared with the most favourable results of former years, the mortality among lying-in women and infants was just one-fifth or one-sixth of what they had been accustomed to. They were entitled to conclude that the method of chlorine disinfection introduced by Dr. Semmelweis had an important influence in producing the more favourable conditions in this Clinic.

Haller was amongst the warmest and most influential supporters of Semmelweis. He was senior physician and assistant Director of the General Hospital, and owing to the respect in which he was held by the staff of that vast institution, his strong advocacy of the Semmelweis doctrine had a determining influence in forming the opinions of all except the staff of the Lying-in Hospital. He concisely stated the experience of former years and mentioned the exact time when chlorine disinfection was introduced "with the consent of Professor Klein," and the method of its application—"before the first examination of a woman in labour to lying-in the hands scrupulously to make clean with chlorinated lime solution and after every examination of a patient, even in the slightest degree affected, to repeat the process of disinfection. . . ."

After calling attention to the diminution in the mortality of newborn infants, and expressing the belief that the experiments made by Semmelweis and Lautner on animals (*directe Versuche an Thieren*) have placed the truth of the theory beyond all doubt (*ausser allem Zweifel*), Haller makes a statement shewing remarkable appreciation of the import of the Semmelweis doctrine, and singular foresight with regard to its possibilities, which is worthy of record and preservation. "The importance of this experience for lying-in hospitals, and for hospitals generally speaking, especially for the surgical wards (*die chirurgischen Krankensäle*), is so immeasurable, that it appears worthy of the attention of all men of science, and it certainly deserves due recognition from the high authorities of State."

Dr. Haller concluded by proposing that Dr. Semmelweis should be invited to give an address on his experiences to the Medical Society. This remarkably flattering proposal was at once adopted, although every member present must have known that the acceptance of such a resolution was equivalent to a vote of censure on Profesor Klein.

Klein had hitherto remained absolutely silent on the subject which so deeply concerned his reputation as director of the First Clinic and as a conscientious man. He still maintained silence, but he took measures to get rid of such an undesirable assistant.

ASSISTANTSHIP EXPIRED.

The proposal of Haller to invite Semmelweis to address the Medical Society was adopted at a meeting in February, 1849; the term of Semmelweis's assistantship was to expire on the 20th of March following, and he resolved to apply for an extension of two years more.

"As the end of my two years' term of office approached I put forward a request for another two years' extension as had been granted to my predecessor. I felt all the more bound to apply for this extension because I hoped during that time to strengthen the evidence in favour of my opinions about puerperal fever by the success of another two years' treatment. However, my request was not complied with, even at the time when my colleague serving in the Second Clinic obtained the favour of an extension. My successor also obtained in due course a prolongation of his term of office for two years more.

"After my retirement from my position as assistant on the 20th of March, 1849, I petitioned for nomination as Privat-Dozent of Midwifery. My request remained without result."

Such are the simple terms in which Semmelweis refers to the close of his official career in the Lying-in Hospital of Vienna, a misfortune for him in his professional career, a disaster to Obstetric Science, a calamity for mankind. He left the scene of his discovery, which stands almost unrivalled for its far-reaching beneficence, dismissed to gratify the spite of his chief against his too zealous friends and supporters.

Semmelweis was so ill-advised as to protest and appeal against his dismissal. This was a hopeless proceeding. There could be no question of the result. The professor was appointed by the Minister of Public Instruction; the assistant was always appointed on the recommendation of the professor. Although Klein's behaviour was highly discreditable, he was quite within his rights when he protested against the interference of his colleagues on behalf of Semmelweis in the affairs of his own Department.

The great excitement among the members of the vast staff of the General Hospital and medical circles in Vienna were only episodes in the professional feud.

Spread of Doctrine by Controversy.

Skoda and Klein.

Skoda had called the attention of the former Minister for Education to the discovery of Semmelweis, but the subject had received no official attention whatever. Skoda was not an easy man to suppress. In January, 1849, he proposed that a Commission should be appointed to investigate the value of the Semmelweis discovery in all its aspects and bearings. The great majority of the Committee of Professors were in favour of Skoda's proposal, but Klein and the other reactionaries, who had as reactionaries much influence in official quarters, were bitterly opposed, and they were successful in preventing inquiry. As a counterstroke a furious attack was made upon Skoda both by clerics and medicals on account of his "materialistic opinions." The proposed Commission was officially declared to be an assault upon the personal honour of Professor Klein under the pretext of scientific research; and if any inquiry was to be made into the incidence of puerperal fever in the First Obstetric Clinic, such an inquiry must be instituted by Professor Klein alone, who had intimate knowledge of the circumstances and knew best the means and the methods suitable for obtaining a solution of the difficulties.

So Skoda's Commission, already nominated, never met, and leaving the investigation to Klein was equivalent to letting the subject drop. He was not the man to throw light on that subject or any other. v. Waldheim remarks very appropriately concerning this lame and impotent conclusion: "Rosas and Klein were the victors in this battle with the Committee of Professors. Stupidity had slain free research, and along with it an already blessed discovery, the prevention of childbed fever would be suppressed in Austria; and with it also another, antisepsis. The year 1849 was the era of reaction, and that speaks volumes."

That may be perfectly true, but it is now of no interest to us except in as far as it affected the fortunes of Semmelweis.

But he was in a sense his worst enemy himself. He had plenty of time at his disposal; he had collected material concerning his discovery, and everything appeared to demand from him some public exposition of his doctrine; but nothing would induce him to address the Medical Society which had given him an invitation so flattering to a young man in his position.

Skoda then advised Semmelweis to occupy his time with experiments on animals, and every facility was given for the purpose. So he and Lautner, Rokitansky's assistant, set to work in a zealous manner upon the dreary research. These experiments are given in detail in the *Ætiologie* and they are mentioned with approval by some biographers, but as a matter of fact they never led to anything. Semmelweis at a later period, even when a grant of money was voted for the purpose by the Society for the advancement of Science, refused to resume the experiments on the ground that the clinical evidence was sufficient to establish the truth of his doctrine—and he was right.

After Haller had completed his report on the work of the General Hospital for 1848 it was found that he had generously devoted several pages to the Discovery of Semmelweis, and he was by no means sparing in his expressions of appreciation.

It was in this Report, part of which had already been read at the Medical Society, that he reached his reasoned conclusion with regard to the "immeasurable importance" of the Semmelweis Doctrine for Surgery.

This was probably the first time in the history of medicine that any suggestion was ever made regarding the importance of prophylaxis, that is, of antiseptics in the practice of Surgery. And this was in the spring of the year 1849. The solemn conventional professors and teachers of surgery at the General Hospital then smiled sarcastically at their colleague, the physician who expressed opinions upon surgical questions; they looked upon Haller as a phantastic enthusiast and treated his inspiration with contempt.

Semmelweis was elected a member of the Medical Society in June of the same year.

The difficulties put in the way of Semmelweis by Klein and his phalanx of sycophants appear to have had for their object to drive Semmelweis out of Vienna in despair. They made statements to his detriment about the work of the First Clinic bordering on falsehood, and Klein refused all access to the records of the department, so as to deprive him of the evidence in support of his doctrine. Still he had with timely industry prepared a large amount of evidence in the form of notes and records of incidents, and as his friends could not persuade him to write or to address the Medical Society, Skoda came once more to his aid. He obtained from Semmelweis all his notes and made a careful study of them, and then prepared an address for the Vienna Academy of Sciences.

Skoda's address was delivered on October 18, 1849. It was entitled: "*On the discovery by Dr. Semmelweis of the true cause of the unusually frequent occurrence of disease among lying-in women in the Vienna Lying-in Hospital and the means of reducing the cases of this disease to the normal number.*"

This contribution to the discussion of the *Lehre* was so important that it is rightly included in the Works of Semmelweis by von Györy, and may be there read in full by the English reader of the German language.

SKODA AT THE ACADEMY OF SCIENCES.

Skoda began by referring to the subject of his address as "one of the most important discoveries in the domain of Medicine." He first of all stated the facts and the conclusions from the combination of which the discovery resulted. He eliminated the epidemic theory, and then traced the history of the Lying-in Hospital in its bearings on the discovery. The various theories of the causes of the mortality are brought under review, just as Semmelweis had discussed them many a time and oft with his friends, and would do again formally ten years later in the *Ætiologie*. In fact Skoda's address contained the essential portion of the *Ætiologie*, and should have made impossible the misunderstandings of the opponents of Semmelweis in after times. If they affected to ignore the humble young Hungarian, they could not overlook a pronouncement by a man in the great professional position which Skoda then occupied.

From the history of the Discovery Skoda went on to the pathology of puerperal fever, and he gave a remarkably clear exposition of the phenomena and symptoms produced by infection. Much of what he said might stand in a modern text-book: all that is wanting is the bacteriology, then unknown, and about the practical value of bacteriological details, beyond what was known from the first to Lister and his early disciples, there is room for much difference of opinion. For example, the following short extract: "From these facts we may conclude that the pyæmia of the puerperal woman develops as a rule from endometritis or *phlebitis uterina*. So we are concerned, in the first place, with the causes of endometritis and phlebitis uterina."

In the course of his address he made a reference to the Lying-in Hospital of Prague, which was the origin of an acrimonious and long-drawn-out controversy. He said: "A well-grounded view to bring the matter into a clear light lay in the circumstance that in the Prague Lying-in Institution the cases of disease were from time to time very numerous, and according to all appearance they arose from the same causes as in Vienna. I urged therefore that the process of chlorine disinfection should be introduced into the Prague Lying-in Hospital. . . . Those who at that institution maintain that puerperal sickness depends upon epidemic influences appear to have got the upper hand, and they seem hitherto to have employed the chlorine washing either not at all or not with serious earnestness."

It was unfortunate for the doctrine that Skoda laid stress on cadaveric poison only as the cause of puerperal infection, and Skoda's great position insured universal perusal of anything that he cared to publish; but it should be remembered that two years had already elapsed since the full import of the discovery had been published to the world in the articles of Hebra. Although the omissions of Skoda were unfortunate, they did not justify the misrepresentations that were founded on them later. Semmelweis saw the omissions at once, but he was too loyal to Skoda to say a word in criticism, or in any way to supplement the address. He could not criticize his champion.

The Imperial Academy of Sciences resolved to include Skoda's address in their transactions, and ordered a special report to be prepared. Semmelweis was at once elected a member of the Imperial Academy. He and Brücke, Professor of Physiology, were offered a grant of money to enable them to pursue their investigations by further experiments on animals, but, as he have seen, Semmelweis considered further experiments superfluous in the view of the amount of conclusive evidence available from clinical observation.

BRÜCKE-SCHMIDT.

Skoda had made representations to men of influence in Prague regarding the application of the Semmelweis prophylaxis in order to reduce the shocking mortality in that city, and thereby brought a nest of hornets about his ears. He roused the animosity of the most vain and self-assertive teacher of midwifery in Europe. We shall see the consequences. Meanwhile Brücke wrote to his friend Professor Schmidt of the Berlin *Charité* with more fortunate results. No one could accuse Brücke of any interested partisanship. He appears to have been animated only by a desire to spread the beneficial truth of the Semmelweis doctrine, in which he sincerely believed. Semmelweis says of Brücke: "He is not a credulous man, but a fundamentally exact investigator." His letter probably conveyed to North Germany the first information about the rise of the new doctrine, but Brücke appears to have mentioned only cadaveric poison as the cause of puerperal fever, the natural result of Skoda's unfortunate statement of the case.

Semmelweis devotes several pages (*Ætiologie,* p. 459) to the Schmidt incident, and it must be regretfully admitted that he is perhaps a little querulous and exacting, and not altogether fair.

Brücke, after stating the case in favour of Semmelweis, had asked Schmidt whether in his experience of practice in the Charité he had met with anything in support of the doctrine of cadaveric infection.

Schmidt replied to Brücke in a perfectly fair and friendly spirit. He described the arrangements of the Charité, which, as a general hospital, contained under one roof every conceivable kind of case of disease, and, as in midwifery work, especially attendance on a primipara, is a tedious business, the students were wont to move about from one department to another while waiting for the completion of natural labour. This practice of alternating between the poles of existence, between the cradle and the bier, between the labour-room and the dead-house, had given rise to certain relevant incidents. . . .

Professor Schmidt is evidently anxious to go as far as he conscientiously can in support of the Semmelweis doctrine out of friendliness to Brücke, but he concludes: "As I have said, I believe in the possibility (of cadaveric poison) and the experiences in Vienna are quite sufficient to make me exercise and require precautions, but I have no personal experience to record. This mode of infection may certainly be one of the many ways in which childbed fever is produced; the only way it certainly is not."

To these remarks Semmelweis makes a long and detailed reply, and falls foul of Schmidt, who evidently desired to be friendly: "If therefore Professor Schmidt has had no experience of his own to support my doctrine, the cause of that did not lie in Schmidt's want of opportunity for observation, but because Schmidt does not possess the ability to make observations." The explanation of this unjust acerbity comes in immediately after a reference to Virchow: it is to be remembered that

Semmelweis was writing the *Ætiologie* nearly ten years after the correspondence referred to, and meanwhile he had not been well treated by Berlin, especially by Virchow. He was human in his resentment, and we may accept his well-founded resentment of Virchow's rather unworthy behaviour as an extenuating circumstance.

BEDNAR AND THE NEWBORN.

Independent testimony in favour of the beneficent influence of the Semmelweis prophylaxis next came from an unexpected quarter. It was from Dr. Bednar, the chief Physician of the Foundling Hospital. When a woman confined in the Lying-in Hospital died, or if she could not suckle her child, the infant was taken just across the Alser-Strasse to the Foundling Hospital. Many of these newborn infants died of a disease indistinguishable from puerperal fever except in the genital sphere. Dr. Bednar, who had a large experience of this disease, called it " Sepsis of the Blood of the Newborn." In the course of a monograph on the subject published in 1850, Dr. Bednar said: Cases of sepsis of the blood of the newborn have now become quite rare. For this we have to thank the discovery of Dr. Semmelweis, lately assistant in the First Obstetric Clinic, whose discovery has conferred such benefits. He was able fortunately to investigate and explain the cause and the means of prevention of puerperal fever, which formerly raged in such murderous fashion in the Lying-in Hospital."

Here then, apart from the fortunes of the Discovery of Semmelweis, is a reference to a now obsolete disease. Still it must be of some practical interest to keep in mind the possibility of doing harm to the infant of a patient suffering from puerperal sepsis, who still retains the capacity of secreting a little milk. What should be done with that milk?

We shall see that as late as 1862 Semmelweis wrote to the Editor of the *Medical Times and Gazette* to correct a statement by Hecker of Munich to the effect that Semmelweis had nowhere explained the mortality among newborn infants owing to puerperal fever of the mothers.

SCANZONI.

Skoda's open championship of Semmelweis had an explosive effect in professional circles in Vienna, and far beyond Lower Austria; and nowhere did it attract more attention and produce greater excitement, resentment and opposition than in Prague. There the circumstances of the University Clinic for Midwifery were, on a small scale, very similar to those of Vienna: the Professor, Dr. Jungmann, had ceased to be an important factor, scientific or practical, and he had an aggressive, self-assertive assistant in Scanzoni, of whom we shall hear much in the controversy of the Semmelweis doctrine. Skoda's reference to the Prague Obstetric Clinic was too plainly derogatory to be tolerated, and Scanzoni set about preparing a reply and vindication. It was only about three years before that Scanzoni had committed himself in a publication to a theory of puerperal fever which was incompatible with that of Semmelweis. Where the truth lay hardly mattered: he must fight.

Scanzoni, then, at the time when Skoda's address was published by Hebra in the Vienna Medical Journal, was assistant to the Professor of Midwifery in the University of Prague. He was one of the last men to accept a new doctrine of puerperal fever, because he had recently acquired distinction by producing a theory of his own. In 1846 he published an article on the genesis of childbed fever which attracted much attention. He then wrote: "The *conditio sine qua non* is the fibrinous crasis of the blood, which, when it rises to a high degree, represents the immediate cause of the disease, and consequently the essential element in the ordinary puerperal fever. It originates under influences of a cosmic telluric character. By no means is the wound of the uterus represented by the placental site the real cause of origin of the puerperal fever."

Scanzoni does not appear to have been a man who would permit his professional reputation to suffer from changing opinions to which he had once committed himself. Whatever might be the evidence produced, to him the frank expressions of adverse opinion contained in Skoda's address appeared to be a personal attack and an outrage, and he proceeded to prepare and publish a vindication (*Rechtfertigung*).

It would not be desirable, even if it were possible, to give anything like a full summary of this portion of the controversy, but enough is introduced to convey some impression of the matter and the method imported into the controversy by one of the most bitter, persistent and unscrupulous opponents of the *Ætiologie* and its author.

Semmelweis (*Ætiol.*, p. 316), after explaining that Skoda's letter concerning the introduction of chlorine disinfection at Prague had been addressed to v. Nadherny, and the reasons for that step, writes: "Scanzoni says: Professor Skoda in the address referred to develops first of all the facts and the conclusions, from the combination of which the discovery of Dr. Semmelweis resulted. This portion of the address offers so little that is new that we do not think it necessary to discuss it further, for it must be known to the greatest majority of the medical profession that the cases of puerperal fever, and fatal cases, are much more numerous in lying-in hospitals than outside."

To this Semmelweis replied that "the facts regarding the mortality were certainly not new, only the conclusions which I draw from the old facts are new. I draw from the old facts the new conclusion that the greater mortality in a lying-in hospital cannot be produced by atmospheric influences. . . . When Scanzoni and 'the legion of epidemicists' allege in spite of the facts that puerperal fever results from atmospheric influences, it is only a proof that they, for want of reflection, have not reached a clear conception of the contradiction which exists between their opinions and the facts, or if they have recognised the contradiction then they only hold on to the accredited errors because they have nothing better to put in their place."

Semmelweis recognised in Scanzoni one of the most able and influential of his opponents, and he devoted a large amount of space in the *Ætiologie* to meet Scanzoni's opinions, and to re-state the evidence in favour of his own doctrine. We have the discussion in the *Ætiologie* running through about 100 pages. It makes rather dreary reading to the medical practitioner or obstetrician of our time, but it has a modicum of historic interest, and as far as Semmelweis is concerned it is deeply interesting in throwing light on his intellect and character. Nowhere better than in this episode is brought out the greatness of the man, his earnestness, his love of truth, his simple sincerity, and his clearness and directness of intellectual vision, in contrast with pedantic antagonists who prevaricate, misrepresent and change their ground, to meet emergencies in debate. We find all these features, unfortunately, in Scanzoni, among the earliest articulate antagonists of Semmelweis, and in many others as the controversy continues.

The intervention of Skoda attracted the attention of the German medical world, and he could not be treated with the disdainful silence and contempt which some had thought suitable for the young Hungarian adventurer. We now note for the first time an element of personal abuse which had not hitherto received frank and open expression from the opponents of Semmelweis.

Yet there was something unreal in the discussion from the first, and it could not bear any good scientific fruit. Scanzoni had committed himself to a fantastic theory of puerperal fever, and he was not the man to admit that he had erred in the slightest degree; and we shall find that he held on to his erroneous opinions until 1867. This theory was not the result of long experience and observation; it could not be.

Scanzoni had a paltry personal grievance about the way in which attention had been drawn to the Prague lying-in hospital, and there was affectation in even that.

"If Professor Skoda had thought it worth his while to obtain more exact information, he would have avoided giving expression to censure which must bring down upon the institution of Prague and its medical staff the suspicion not only of the medical profession but also of the public. We know that in Vienna some time ago . . . . a rumour was widely spread that the mortality in the Lying-in Hospital of Prague was remarkably high. . . . . It was said that the alleged shocking results were owing to our indolence and stupidity in not introducing the chlorine disinfection of Dr. Semmelweis. We said nothing about these accusations, because we expected that those who spread them would consider it their duty to formally call the attention of the government to such criminal behaviour, in order that an investigation might be made into the facts. . . ." Then follows a justification of all that they have done at Prague, and statistical proof of how successful they have been. But the cases of puerperal fever at Prague were counted on a purely arbitrary basis, and the statistics were worthless and misleading. From the category of puerperal fever Scanzoni excluded the cases which we now universally recognise as the most dangerous forms of puerperal sepsis, *e.g.*, endometritis septica and metritis. The defence and counter-attack are mere logomachy, and serve nothing but personal ends. According to Scanzoni, although the directors of the hospital at Prague had not received any direct communication from Skoda in a manner suitable to the importance of the occasion, a fair trial had been given to the Semmelweis disinfection; "very few visits were made to the post mortem room, and the prophylaxis was applied and supervised with the greatest care. But the number of cases did not diminish in the slightest degree in spite of all our efforts; the unfavourable state of things continued until a more favourable *genius epidemicus* relieved us. The number of cases diminished suddenly so that in the month of May there was only one death among 205 patients, whereas in March and April we had 31 deaths accidentally (zufällig) among 406 puerperæ in spite our chlorine disinfection."

In the same 'hoch' style Scanzoni goes on to propose that the "*k. k. Landesgubernium*" of Bohemia should appoint a Commission to inquire into the reproach thrown from Vienna at the lying-in hospital of Prague to the effect that the sickness and mortality there were entirely owing to the indolence and inefficiency of the staff. Every member of the medical profession would be interested to see some light shed on the question of the etiology of childbed fever. "If the malady is really contagious then all the lying-in hospitals must be considered state-supported murder-dens." If "*k. k. Landesgubernium*" would appoint such a Commission and solve this highly important question it would confer an undying service to humanity and to Science.

As at Vienna the farce of appointing a Commission was performed in 1849, and as at Vienna the old professor of midwifery, Herr Professor Ritter v. Jungmann, prevented it from meeting on the ground that they must wait for an epidemic in order to make observations.

In the course of a reply to Scanzoni which Semmelweis wrote nearly ten years after he says *inter alia* :—

"Scanzoni made the experiment of using chloride disinfection for six months, and drew conclusions to the effect that chloride disinfection could not have much effect, and that the frequency with which patients were attacked did not depend upon cadaveric poison.

"The reader knows the weighty grounds on which Scanzoni opposes me, and he may be sure that Scanzoni has not remained silent concerning these in the presence of his students, and that consequently they did not exercise the care upon conscientious disinfection that was necessary. Among my students such men were found in spite of my urgent requests with regard to disinfection.

"Scanzoni may perhaps plead ignorance since he speaks only of cadaveric poison as the cause of infection and seems to think that the practice of making autopsies extremely seldom was sufficient precaution. But the great source of infection (Die grosse Quelle) is the patient whose disease produces decomposed material, and if stringent rules concerning such patients were not laid down for the guidance of the nurses the efficiency of the chloride disinfection would be impaired.

"Then there is a third source of infection of which we have had experience in Pesth in spite of all my watchfulness, viz., filthy bed-clothes and linen and careless nursing.

"If I were in Scanzoni's place and he were in mine I could not allege with complete truthfulness that I had made assiduous use of the chlorine disinfection, and consequently the conclusion that puerperal fever did not result from the resorption of decomposed matter could not be considered proved. . . .

"Scanzoni would find himself embarrassed if he were asked to give his authorities for an etiology of puerperal fever which explained how in the month of March and April, 31 women died by chance out of 406, and how this accidental mortality was reduced to one death by a more favourable *genius epidemicus,* and how "without any demonstrable cause" the deaths rose again to 9 in June, 2 in July, and 8 in August. . . .

"Scanzoni constantly speaks of a hypothesis, but he has nothing to object to the facts and conclusions on which it is founded except that they are not new; that the facts are not true or the conclusions erroneous Scanzoni has nowhere produced the slightest evidence. . . .

"He calls my discovery a hypothesis. . . . We understand the grounds on which our discovery made in Vienna is degraded into a hypothesis at Prague; it is because Scanzoni does not understand the essential points of the discovery; of the three sources of decomposed matter he knows only one. . . .

"Scanzoni declares that he does not agree with us, but he states no reason for not agreeing. . . .

"Scanzoni will never take a position of honour in the history of puerperal fever, not because he opposes me, but because of the method of his opposition. The object of his opposition has always been to prove that he himself was in the right, and in order to attain this object he has gone the length of denying the truth. . . .

" What Scanzoni says about the forms of puerperal fever we can read in many textbooks on midwifery but we never observe such things in Nature. . . .

"The longer I think over your efficiency as a clinician at Würzburg the more likely it appears to me to be that your opposition to me has not arisen so much out of ignorance (Unwissenheit) as out of ill-will (aus bösem Willen) for you have within six years lost only 20 patients from puerperal fever. The proportion of your mortality to that of Kiwisch is then as 24 to 432. How do you obtain such favourable resuls? Are you, Herr Hofrat, privately such a fortunate observer of the details of my method, and only pose in public as my antagonist? Under what mask have you smuggled my teaching into the Würzburg Lying-in Hospital? Do you live, Herr Hofrat, in the belief that you can only shine when all around you is dark? Do you build your greatness in making fools (Verdummung) of those who come to you for teaching? If so then you build your greatness on the corpses of the unhappy lying-in women whose deaths have been caused by the pupils whom you have befooled."

As v. Waldheim (p. 63) says, in reviewing the position taken up by Scanzoni: " It was no longer a scientific encounter, no struggle to attain to a knowledge of the truth, but a thoroughly personal, hateful wrangling under the semblance of scientific discussion. Only pure thought brings progress to the scientific investigator. If personal considerations influence him, then is his impartiality at an end. . . . He is no longer a champion of the truth, but in order to appear to come off victorious, and for that end alone, no means can be too vile."

Scanzoni, who became Privat-Dozent of Midwifery at

Prague, and was soon to be called to Würzburg as Professor of Midwifery on the death of Kiwisch, left the completion of his reply to Skoda in the hands of Seyfert, his successor as assistant in the lying-in Hospital at Prague.

Seyfert.

In all the mad controversy and emotional detraction there is nothing to rival Seyfert's contribution in monumental self-satisfaction, obscurantism and imperviousness to new knowledge. But he is only a stipendiary echo earning the approval of his chiefs; otherwise he would not be worthy of a moment's consideration. There were very few young men then living so filled with the enthusiasm of humanity and the love of scientific truth, as to disregard professional advancement like Semmelweis; Chiari and Arneth probably were strong men who dared to speak out: the rest were nowhere.

Seyfert begins with the same silly complaint as Scanzoni, that they had received from Vienna no *direct* invitation to give a trial to chlorine disinfection: they had, however, heard of it from visitors who had been to Vienna, among whom was certainly our Dr. Routh, though Seyfert does not mention him by name. At Prague they had apparently never heard of Hebra's articles. They had enough intelligence and good feeling (Verstand and Herz) to give a trial to a process from which they had been led to expect so much. Unfortunately he cannot report any favourable result; certainly there were not more cases of puerperal fever than before the introduction of chlorine disinfection. Then follow some small sarcasms about Skoda's expression "the usual number." . . .

In spite of the disinfection the mortality increased to an important extent, " as it always does in the month of March." Before the number of cases increased in the Lying-in Hospital " an important epidemic of puerperal fever had prevailed in the town in the month of February, with a much higher mortality than in the Lying-in

Hospital. With the return of better weather the puerperal fever epidemic diminished, as happens every year." We shall hear more of this hitherto unheard-of phenomenon of epidemics among the population of the town in which the Lying-in Hospital was situated.

In spite of our avoidance of immediate contact with the cadaver in post-mortem examinations and the diligent use of chlorine disinfection numerous cases again occurred in January, 1850, " with a sudden change in the temperature." In November, 1849, out of 208 women confined 10 died; in December, 1849, out of 215 women confined 16 died; in January, 1850, puerperal fever attacked 30 patients, and 22 died. " *These figures as compared with former years show no special difference.*" . . . . " To say that the putrid material cannot be washed away from the hands by means of soap and water seems a bit of an exaggeration. Chlorine as a disinfectant seems to us an *arcanum*. . . ." Semmelweis, remarking on this passage, says: " Seyfert denies that chloride of lime possesses any disinfectant properties. It seems ludicrous to name Seyfert as an authority against Liebig on such a question. I did not discover the disinfectant properties of chloride of lime; I only brought them into use."

Seyfert continues his experience and reasoning thereon. The infectious matter must produce a local inflammation which must spread by the lymphatic vessels and the veins so as to produce pyæmia. We have never seen in a post--mortem examination any evidence of such a process affecting the genitals. . . . . Finally, it is surprising to learn that Skoda finds puerperal fever to be the same disease as pyæmia. . . . " Endometritis does not in any way belong to the characteristic symptoms of puerperal fever." He also denies the presence of wounds in the genitals unless they existed before labour. Altogether a very remarkable exposition of opinion from an aspiring young teacher of midwifery in the year A.D. 1850. It is interesting only as indicating what was taught at Prague by Scanzoni, the relentless opponent of Semmelweis and his *Lehre*, and as illustrating the lengths to which the personal partisan spirit could be carried by men who were supposed to be seeking for scientific truth.

Scanzoni next appears in print in a quarterly medical journal published at Prague in the last half of the year 1850. After expounding on a previous occasion all the ancient notions about the causes of puerperal fever, such as chilling, mental excitement, wounded modesty, etc., he now categorically denies the statement of Semmelweis that the midwives do post-mortem examinations at the Paris *Maternité*, and he alleges that the *Gassengeburten* (street-births) in Vienna are as much followed by puerperal fever as any other. He also stated, like his colleague Hamernik, that puerperal fever, is more feared in England than anywhere else in the world, because there great and murderous epidemics frequently occur. The object of these reckless and untruthful allegations is to support his theory that "the conveyance of cadaveric poison as a cause of puerperal fever is an erroneous and arbitrary statement" (ist durchaus irrtümlich, ist durchaus willkürlich).

WIEGER, of Strassburg.

At the time when Semmelweis was making his first trials of chlorinated lime as an antiseptic in the First Obstetric Clinic, one of the young foreign graduates who repaired to Vienna for experience, was Wieger of Strassburg. He was visiting Vienna at the same time as Routh, who refers to him as an acquaintance, and quotes his experience of the ravages of puerperal fever in the Strassburg *Clinique*.

Wieger took an eager interest in the prophylaxis of Semmelweis, and impressed with the success attending it, he resolved to make the doctrine of Semmelweis known in France. So on his return home he published an article entitled: *Des moyens prophylactiques mis en usage au grand hôpital de Vienne contre l'apparition de la fièvre puerpérale*. This was in 1849. Wieger's efforts were not crowned with success. In the *Union Médicale* his article was published under the heading of "doubtful anecdotes," amidst pleasantries about the uncleanly habits of Vienna students, and the autopsies made by Rokitansky. How any one could see material for amusement in the grave and earnest bearing of Rokitansky engaged in his professional work, it is hard to imagine: it seems to argue a singular levity in the editorial staff of which Semmelweis was also the victim.

At the request of Wieger, Semmelweis sent, in the following year, a communication to the *Académie des Sciences,* but he never received any acknowledgment.

Wieger now considered it his duty to publish independently the article just referred to. He analysed the mortality of the Vienna Hospital, as Semmelweis ultimately did in the *Ætiologie,* traced the cause of puerperal fever, discussed contagion, the question of miasma, of overcrowding, of the *genius epidemicus* in every detail. His reason for writing was his conviction of the truth of Semmelweis' teaching, and as a protest against the manner in which it had been received in France. Wieger's work contained all and more than was brought forward as independent opinion, in the four months' discussion at the *Académie de Medécine* at Paris ten years later, but he was a comparatively young and unknown man, and the seed fell on stony ground. Wieger's chief, Professor Stoltz, at first opposed the Semmelweis doctrine, and had much to answer for in preventing the method of prophylaxis from being adopted in Eastern France, just as Dubois in Paris delayed the adoption of the methods of prevention advocated later by Tarnier. Antisepsis, asepsis, and isolation were things *pour rire* for Dubois and his compeers.

ADDRESSES IN THE MEDICAL SOCIETY.

Difficult as Semmelweis was to move he must at last speak out. So he overcame his diffidence and gave his first address at a meeting of the Vienna Medical Society in May, 1850. He was still waiting for an answer to his petition for nomination as *Privat-Dozent* of Midwifery. We have only the minutes of the meetings to inform us about the proceedings.

He went over the well-worn ground of the comparative history of the two Clinics, and refuted once more the allegations about endemic factors, overcrowding, puerperial miasma, etc., in the etiology. The history of the Lying-in Hospital showed that from its foundation in 1784 until the anatomical basis of medicine was made a feature of the Vienna School of Medicine no serious epidemic of puerperal fever had occurred in the Lying-in Hospital, and that the mortality in Boër's earlier days never once rose to over 1 per cent. per annum, whereas under Klein there had been periods when almost one patient out of every two was carried off by the malady.

After his exposition of the cause of puerperal fever and clearing away erroneous statements, Semmelweis said : " Puerperal fever is therefore as little a contagious disease as it is a specific disease in itself : it develops in this way, that an animal organic material which has become putrid, whether originating with a diseased living organism or from the cadaver, taken into the blood-mass of the puerpera, produces a puerperal pyæmic blood-dissolution (*Blutentmischung*), whence result the well-known exudations and metastases."

Yet in spite of this clear and complete exposition of the new doctrine, once more repeated and published in 1850, we shall find it constantly alleged twelve or fifteen years later that Semmelweis attributed puerperal infection to cadaveric poison alone. Curious testimony to the care with which professors of midwifery read their medical journals in those days by way of equipment for the conscientious performance of their duty as teachers. It was at this meeting that Semmelweis mentioned for the first time unclean utensils as conveyors of infection; but this also was overlooked by the unsympathetic.

In June of the same year Semmelweis devoted the second address mainly to replying to Scanzoni and

Seyfert. He took these antagonists far too seriously, and in dealing with others he showed a remarkable want of the sense of proportion. By no incident is this better illustrated than in his laboriously conscientious reply to the disingenuous and cowardly attack of a Dr. Zipfel, who had been formerly assistant in the Second Obstetric Clinic, and had been recently raised to the rank of *Privat-Dozent.* This individual had during his term of office done much dissecting, and the mortality in his Division had been unusually high, from 12 to 15 per cent.; he had given Semmelweis permission to make use of the facts, had congratulated Semmelweis on his discovery, to whom he confided that he had very nearly made the discovery himself! He now suddenly changed his expressions of opinion, evidently under influences not far to seek, and attacked Semmelweis in a violent and offensive manner. Semmelweis might well have treated him with silent contempt, but he devoted the same conscientious care and labour to the refutation of Zipfel as if he were debating with men of the calibre and position of Scanzoni and Kiwisch.

It was Zipfel who had transmitted to Prague the statement that *Gassengeburten* were as often followed by fatal puerperal fever as the ordinary hospital cases, a statement used by Scanzoni as evidence although it was a gross misrepresentation of facts.

It was natural, considering his own record, that Zipfel would not willingly admit that cadaveric poison was a cause of infection in childbed. Semmelweis also took the trouble to seriously discuss the claim, now at last announced by Zipfel, that he and 'Fergusson'' were the discoverers of the true cause of puerperal fever, and they were of course prior to Semmelweis.

Zipfel appears to have read some translation from English, or obtained in conversation with a visitor from the United Kingdom, some confused notions about the English theory of contagion. The reference is probably to Dr. R. Ferguson of King's College Hospital in London whose work on puerperal fever was published in

1839. As we shall see Ferguson's theory was the subject of remarks in the discussion at the Obstetric Society of London in 1875; needless to say it bears little or no resemblance to the Doctrine of Semmelweis.

The complete change of front on the part of Zipfel, the whole nauseous incident, is mentioned in illustration of the lengths and the depths to which some of the mercenaries of debate were willing to go in order to curry favour with their seniors by misrepresentating and belittling the man of genius whom they envied, but could not understand, and therefore must treat disingenuously and spitefully.

It was in the reply to Seyfert on this occasion that Semmelweis first called attention to the need for the application of chlorine disinfection to gynæcological surgery.

The putting of this theory into practice was left for Semmelweis himself years afterwards in Buda-Pesth.

In the meeting of July 15, 1850, Dr. Chiari, who had also been an assistant in the Lying-in Hospital, spoke of Semmelweis and his services to medical science in the most generous and enlightened manner. This praise of Semmelweis was a courageous action on the part of Chiari, for he was Prof. Klein's son-in-law, and Klein was probably sitting in the meeting listening to the discussion but taking no part in it.

Dr. Helm, the temporary Director of the General Hospital, who had also been an assistant in the first Obstetric Clinic, spoke in indignant terms of the attempt to rob Semmelweis of his priority as the discoverer. He explained the prevalent English opinions, and declared that no one before Semmelweis had expounded the etiology of puerperal fever in such an exact and definite manner, and Semmelweis had also the unique merit of introducing the measures by which infection could be prevented. He classified and gave some account of the various opponents of Semmelweis, and finally declared that every individual medical practitioner, as well as every medical corporation, was under a deep debt of gratitude to Dr. Semmelweis.

Dr. Arneth, then assistant in the Second Clinic, declared his conviction that it was contact with the cadaver which was the only factor in causing a greater danger to patients of the First Clinic in comparison with the condition of matters in the School for Midwives. In every other way the advantages lay with the School for Students of Medicine. In answer to some paltry objections such as were raised by Levy of Copenhagen, that no one had traced the infection in any given case, he replied that before Semmelweis's discovery no one had thought of such observations, and now no one would take the grave responsibility of discarding the prophylactic washing in order to make observations upon suffering women. In illustration of some of his points, Arneth mentioned the experience in English private practice of Dr. Joseph Clarke, of Dublin, who had attended 3878 confinements and lost only six mothers from puerperal fever. He joined in the tribute of praise to Semmelweis: he had not only brought a new idea to light, but what was the most important fact about the discovery, he had found the means of bringing prophylaxis into rational and successful application, and had proved its value.

Some remarks from Rokitansky, who presided, brought to a close the most important discussion that had ever taken place in the Vienna Medical Society. It was for Semmelweis a perfect triumph: his friends Chiari, Helm, Arneth and Rokitansky spoke in his praise, and among the sympathetic listeners were Skoda, Hebra, and Haller. A pathetic figure must have been presented by Klein, who probably sat there silent throughout the whole three nights' discussion. Skoda and Hebra had at first tried to convert him to the new doctrine, but in vain. He had always been a dull man, had always occupied the ridiculous undignified position of a man pitchforked into a chair which he had not the capacity to worthily fill. Now he was growing old and more obviously than ever incapable of assimilating new ideas. He could only feel jealousy and aversion, and that

Semmelweis was finding to his cost. To the great satisfaction of his friends Semmelweis had made an excellent impression as a speaker. Now that he had successfully made the first step in public utterance much was expected from him. But even after this triumph Semmelweis once more made a fatal mistake. He trusted to the publication of the Minutes of the Meetings of the Medical Society instead of writing out his addresses in full, and publishing them to the whole world. His opponents were much keener and less costive and reticent. They had now the arena to themselves for many years, and they took full advantage of their opportunities.

Kiwisch, of Würzburg.

In order to save the feelings of the staff of the Lying-in Hospital at Prague, Skoda had arranged that the letter inviting them to give a trial to the chlorine disinfection should reach them through v. Nadherny, a man whom he had every reason to believe was *persona grata* at the University of Prague. Hence the paltry reiterated complaints that they had received no *direct* request from Skoda or Semmelweis.

As Skoda's letter was not well received at Prague, v. Nadherny sent a copy to his son-in-law, Kiwisch von Rotterau, then Professor of Midwifery at Würzburg. Kiwisch was then considered one of the chief obstetric authorities in Germany: we shall learn from his "Some Words" what manner of guide to obstetric science he proved to be.

His contribution to the discussion was entitled: *Some Words concerning the discovery by Dr. Semmelweis of the origin of puerperal fever published by Dr. Skoda.* The essay of Kiwisch was published in the *Journal of the Medical Society of Vienna* soon after the meeting of July, 1850.

Kiwisch said that during his visits to Vienna in 1848 and 1849 he became acquainted with the opinions and the practices of Dr. Semmelweis; and we may be sure he associated much with the opposition. What he saw and heard excited the highest interest, and induced him to institute fresh investigations. He knew how to appreciate the services of Dr. Semmelweis, but he could not express himself as agreeing with Dr. Skoda in his opinions when he speaks of a new discovery resulting from the inquiry in question. The opinion that puerperal fever is produced by infection from decomposed animal matter, and especially by cadaveric poison, has been advanced for many years, and in many quarters it has received an animated defence; and it would have obtained general acceptance long ago if its supporters could have produced anything like sufficient proof. . . . That Dr. Semmelweis has with great perseverance, and as it appears with so much success, endeavoured to diminish the mortality in the Vienna hospital and has thus done great service, no one would call in question." Then Kiwisch unconsciously demonstrates, under some display of knowledge of English obstetric literature and practice, that he has entirely mistaken the discovery of Semmelweis for the doctrine of a specific contagion generally accepted in England, just like Simpson at first. "Some Englishmen went much further than Semmelweis in their efforts at prevention, inasmuch as they not only considered their fingers as the carriers of the infectious material, but also their clothing; they not only washed their hands with chlorine water, but fumigated their clothes or changed them, or finally if that was not sufficient they left their practice for a time." Then follows a long list of the often quoted English cases which led to the theory of contagion in England, and the usual story of epidemics in rural districts which could not possibly have originated in infection by decomposed animal matter. In all this there is just a suspicion of modification of the English theory, as usually accepted, in the direction of the Semmelweis doctrine which had just been announced in London by Routh. Storrs, *e.g.*, who is quoted, was certainly a contagionist and nothing more.

Referring to Skoda's quotation of a statement by

Kiwisch that he examined frequently (nicht selten) pregnant and puerperal women immediately after doing post-mortem examinations, and had not observed any disadvantage from so doing, that no consideration was given to the question whether students went direct from the dissecting room to the labour ward or not, yet during the last two and a half years the health of the puerperæ has been very satisfactory (*sehr zufriedend*). Kiwisch slightly modifies his statement. He has to admit however that in 1846 they had suddenly a frightful outbreak of puerperal fever, and that in spite of all precautions it did not cease until "the warm time of the year." It did disappear then, although the methods of clinical examination remained the same.

Out of 102 women confined 32 sickened, and 27 of these died, that is to say more than 26 per cent. The disease was of a specially virulent type; and it arose obviously without any cadaveric infection. . . . . The reason for the sudden outburst could be sought for only in atmospheric conditions. . . . . There were repeated outbreaks, and these "always coincided with changes in the weather." All precautions were in vain, "until suddenly in the month of July, without any change in the conditions within the hospital, the health of the patients recovered the ordinary satisfactory state." . . . Considering the multiplicity of injurious agents which may produce or favour an outbreak of puerperal fever, it is extremely difficult to estimate the influence of one injurious factor; nevertheless every director of a lying-in institution, as well as every medical practitioner, should take every available means to prevent the access of cadaveric poison which may infect lying-in women as the practical application of the results obtained by the investigations of Dr. Semmelweis." (See also *Ætiol.*, p. 429.)

It is difficult to assess the value of this contribution to the discussion. It will be observed that Kiwisch is quite satisfied with himself, although he had for some years a high mortality reaching 26 per cent. in one year at least.

Kiwisch admits that some cases of puerperal fever may be produced by cadaveric poison or some other decayed animal matter, but he does not say a word about the introduction of methods of disinfection into the Würzburg Lying-in Hospital. In reading the article of Kiwisch it is impossible to avoid the impression of a certain *ex cathedra* air which pervades it, as if he said "when I pronounce my judgment the last word has been said on the subject." He assumes also, while making certain concessions, a patronising professorial attitude which must have been intolerably galling to Semmelweis still a petitioner for appointment as Privat-Dozent. But whatever we may think of Kiwisch, Scanzoni, and other opponents, we cannot overlook the fact that they were becoming the leaders of professional opinion in Germany, unhappily for Semmelweis and his aspirations.

The best judgment on Kiwisch in his attitude towards the problem of the etiology of puerperal fever is without question that of Hegar; it is just but not unfriendly. "If the history of human error had not already exhibited sufficient examples we might have been astonished to find that such a genial man and such an experienced obstetrician could be so prejudiced and blind." After quotation of the remarks of Kiwisch about his entire want of precautions with regard to parturient and puerperal women and the absence of any unfavourable results, Hegar goes on to say: "This was written by the self-same man under whose direction the Lying-in Hospital of Würzburg had shown a mortality of 26 per cent. in one year."

LUMPE. It is a relief to turn from the solemn pragmatic pronouncements of Kiwisch to Dr. Lumpe, whose contribution is conceived unintentionally in a lighter vein. He illustrates the well-worn saying that fools rush in where angels fear to tread. We may also sadly reflect that there were not very many obstetric angels among the professoriate and official staffs of the lying-in hospitals of Germany and France in those days. We may therefore try to "suffer fools gladly" for a change.

Lumpe had also been formerly an assistant under Klein at the First Clinic. In the great debate of July 15, 1850, he had given the meeting the benefit of his opinions; he appears to have had the by-no-means rare faculty of rendering obscure what all men thought they understood before hearing his exposition, and of coercing facts into squaring with theories. Possibly stimulated by coffee-house *badinage* about his last appearance, he returned to the subject by writing "A THEORY OF PUERPERAL FEVER." The theory of Semmelweis is like the egg of Columbus, it is too simple to be conclusive. When he was assistant in the First Clinic and witness of the sad ravages of childbed fever, he had many doubts about the sufficiency of the explanations then offered on the origin and prevention of the disease. They appeared to be logical contradictions on which the *pia desideria* of Humanity in the field of exact Science could not obtain a foothold . . . If cadaveric poison is the cause of the disease, as Semmelweis declares, the effects must be in a direct and definite relation to the cause, consequently the more frequently the infection is brought into contact with the patient by means of examining fingers, etc., the more frequently must cases of illness and death occur, and *vice-versa*. If there is an etiological relationship it cannot be the reverse of all this, and the disease become more rare the more frequently direct contact occurs.

Lumpe says he was highly pleased when he heard of the happy results obtained from chlorine disinfection, but he had some doubts, and instead of studying these doubts in order to solve them he accepted the doubt as fact, and the undoubted fact of the results of the disinfection as false, merely the result of accident! That is logic, that is exact science according to Lumpe.

The writer then goes on to shew that when he was assistant the cases of puerperal fever were most numerous just in those months when he did not give a course of operative midwifery on the cadaver, and least numerous when he carried on one or even two courses. "Many a time this result was demonstrated that the oftener the chances of conveying infection occurred the smaller was the number of patients attacked"! After this masterpiece it is hardly worth while to quote more, but a few words may illustrate further the extremes to which local and personal influences could carry even a good-natured man. The First Clinic did not have such bad results compared with the Second, after all allowances are made for the want of ventilation: puerperal fever occurs in the epidemic form outside the lying-in hospitals, and is increased and spread by a miasma: and he will not believe to all eternity (in alle Ewigkeit) that the examining finger impregnated with cadaveric poison is in any special way the conveyer of infection. Still he would not go the length of saying that chlorine disinfection is superfluous . . . Whether or not cadaveric poison may possibly be the least of all the contending factors in the production of the disease, the future must decide. "Meanwhile, we must wait and wash." (*warten und waschen.*)

v. Waldheim says of Lumpe, probably from the oral testimony of men who knew him: "An imaginative and ready orator . . . a sympathetic character, an honourable antagonist, a learned head—everything that was agreeable, but no scientific investigator, incapable of observing Nature, deficient in that piercing intellectual vision which enabled Semmelweis so readily to solve the problem which had been so long an enigma to all the world."

On the question of "the egg of Columbus" simplicity of the revolutionary theory of Semmelweis, Bruck makes some pointed and thoughtful observations:

"With a new thought he had come upon the stage. . . There since men began to think they had been accustomed to stand helpless in the presence of puerperal fever. In order to explain this helplessness men had been accustomed to call in atmospheric, cosmic, telluric influences, and considered a change in the weather as a decisive factor in the outbreak or the absence of the disease . . . All this had been with airs of importance pronounced *ex cathedra* to the world, and at the sick-bed, and now in the turn of a hand it had all become false and misleading. It had to be confessed that all that had been taught for years, about which thick-bellied books full of learning had been written, was error throughout; that a small piece of chloride of lime was sufficient to throw upon the scrap-heap the whole learned apparatus which so many distinguished men of science had been collecting and elaborating for centuries, with the industry and perseverance of bees; that the application of chloride of lime was sufficient to arrest an outbreak of the disease against which all efforts had hitherto been put forth in vain. All that appeared to be too simple to be seriously accepted."

BAMBERGER. Among the curious incidents of the discussion at this stage was the intervention of Prof. Bamberger, then of Würzburg. Bamberger was professor of internal medicine, and could not be expected to give expert opinions on the clinical phenomena of puerperal fever. Doubtless he wished to oblige his friend and colleague, Prof. Kiwisch. Bamberger began by asserting that endometritis is only the local expression of a blood-disease; he summed up his conclusion under four heads, and he was utterly wrong in every statement as the experience and observations of half a century have shewn. One of his points is worth mentioning because it indicated an error to which the opponents of Semmelweis were all committed, viz., *that the general symptoms of the blood-disease preceded the local processes.* No doubt Bamberger meant to do his colleague a good turn, but *ne sutor ultra crepidam.*

CLOSE OF VIENNA PERIOD.

In February, 1850, Semmelweis had petitioned for the second time for recognition as *Privat-Dozent* of Midwifery. They kept him waiting till October, and then he received nomination as Privat-Dozent for Theoretic

Midwifery, *with restriction to the use of the phantom in teaching,* and in the list of classes for the winter session, 1850–51, this notice actually appeared: "Lectures on Midwifery with practical demonstrations on the phantom five times a week by Dozent Ignaz Semmelweis." So the *venia legendi* was not granted in full. What was offered would not enable him to grant certificates of attendance like the professor and the other *Dozenten.* There was really a good vacancy for a teacher of midwifery. The predecessors of Semmelweis had been mostly called as professors to other universities, and some were going abroad, as, *e.g.,* Arneth, who was then about to visit the British lying-in hospitals. There was a most promising career opening for Semmelweis: he was backed by all the most distinguished men in the Faculty of Medicine, on whose side stood the future, but his record as a revolutionary was against him, and he had incurred the jealous hatred of Klein and Rosas; that was sufficient to ruin his career, at least at the start. It was discovered later that there must have been some element of treachery in the scheme to humiliate Semmelweis, for in the Act of the Minister of Education appointing him *Dozent,* the terms were distinctly stated "with practice on the phantom and cadaver." This discrepancy has never been explained. The falsifier had no doubt gone to his own place long before Semmelweis had become of sufficient eminent in reputation to lead any one to investigate this final wrong in Vienna.

The insulting condition attached to the nomination aroused a feeling of bitterness in Semmelweis which grew and rankled. "His patience was at an end; his anger rose beyond the bounds of reason, and evidently remembering only the persecution of enemies and forgetting what had been done for him for years by a circle of loyal and devoted friends and supporters, he formed the rash and irrevocable resolution to leave Vienna, which he loved so well, and to return to Pesth, where he was now a stranger." So he went off without a word of farewell to any one of his professional friends, not even to

Skoda, who had done him so much disinterested service, and had made so many enemies in his generous struggle to establish the doctrine of which he was among the first to recognise the truth.

Skoda, as was to be expected, was deeply hurt by the ingratitude and folly of Semmelweis. He said nothing, but for him Semmelweis ceased henceforth to exist.

Many surmises and conjectures have found expression as to this unexpected action on the part of Semmelweis. It has been suggested that it was the earliest expression of the mental aberration which ultimately developed. That explanation appears to be rather far-fetched, for Semmelweis had still before him twelve to fourteen years of active professional work.

Fleischer was probably right in his surmise that the political and social influences employed by Klein and his sycophants were too strong for him. Semmelweis would be speculating beforehand and dreaming dreams as to the spread of his teaching, if only his application for the opportunity to teach should be successful. He would be aware of Klein's opposition, but he no doubt exaggerated the extent of the recognition of his discovery, and he naturally minimised the effects of his exploits as a revolutionary on his prospects of promotion.

At that time the most influential person in advancing and retarding the careers of aspirants to distinction in the Medical School of Vienna was Professor Rosas. He was the leader of the clique of obstructives, which included Klein. He is described as the embodiment of reaction and militarism, always ready to exercise all his influence against the aspirations of any young man of ability and originality who exhibited the slightest spark of independence.

It may be apposite to mention here a remark made by Kussmaul in his "Youthful Memories . . ." when giving some impressions of his visit to Vienna. "In Austria a revolting system of favouritism dominated everywhere: there were incompetent professors and chief physicians who owed their appointments to the patronage of distinguished petticoats and influential cowls."

So Semmelweis appears to have at last recognised that the memory of the bureaucratic reactionaries was not so short, that he was under the eye of the police, held in suspicion as a potential rebel and in contempt as a Hungarian patriot, and until the times changed there was no hope of promotion for him in Vienna. It may be said in view of future developments of opinion with regard to puerperal fever that it was a great loss to the Vienna Medical School not to have Semmelweis attached to the hospital; that, no doubt, is quite true, but they were accustomed to do such things in the Vienna School. They had there to bear the reproach of arresting the development of percussion in diagnosis by neglect of Auenbrugger's work until Corvisart translated it into French and attracted to it the attention of Europe. And we have just seen how official persons arrested for a time the use of auscultation by making impossible the pioneer work of Skoda in the General Hospital, and sending him first to practice in a lunatic asylum and then to seek comparative liberty as a police surgeon.

Disappointed and disheartened as he was Semmelweis appears to have hesitated a little, because his name, with his subject, was printed in the prospectus of medical courses for 1850–51, "Lectures on Midwifery with practical Demonstrations on the Phantom . . . by Dozent Dr. Ignaz Semmelweis." Then, perhaps in contemplation of this very "Lectionskatalog," came a fierce outburst of the Hungarian temperament, enforced by some hereditary idiosyncracy of recklessness such as is suggested by the patriotic action which also ruined the career of three of his brothers. Whatever may have been the causes or the motives at work, all that he says himself on the subject is contained in the few words: "*Ich übersiedelte daher noch im Monate October* 1850 *in meine Vaterstadt Pest.*" (*Ætiologie,* p. 81.)

V.

Life in Buda-Pesth.

State of Hungary.

The resolution to return to his native city was the great turning-point in the career of Semmelweis. The time chosen for the momentous decision makes the step all the more inexplicable, even mysterious. If he had returned to take part in the revolution or the civil war in which his whole family were more or less involved, the rash proceeding would have been more intelligible, but he returned to his country when no struggle, military or political, was any longer possible. Hungary was in the position of a conquered nation, and society in Buda-Pesth was disorganised.

This is no place to digress into the history of the period, but a few words must be written in explanation of the state of Hungary in general and Buda-Pesth in particular, relevant to the influence of events upon the career of Semmelweis.

The movement for political freedom and rights as a nation had been proceeding in Hungary for decades, not to say generations, but the people as distinguished from the magnates, greater and less, were no more prepared at the beginning of the century for the exercise of political rights than the peasantry of France at the time of the revolution in 1789. Progress intellectual and industrial had a long way to go before the Hungarian populace were ready to make reasonable use of political rights and privileges. The Emperor Joseph II. had presented the extraordinary phenomenon of a reforming monarch, just as Europe was to witness later the advent of a reforming Pope. He endeavoured to thrust political privileges and religious equality upon Hungary, but his peculiar methods, combined with his policy of Germanising everything, caused such resentment that the people preferred their grievances to the Emperor's reforms. After Joseph's death in 1790 there was a short period of peaceful progress, and then came the terrible reaction consequent upon the French revolution and the murder of Marie Antoinette, daughter of Maria Theresa. The Napoleonic aggressions still further hardened the heart of the Emperor Francis, and the government of Hungary, by the Austrian autocracy, became a grinding tyranny. Education was especially discouraged.

Political agitation in Hungary, although unremitting, was not marked for decades by any historical incident of the first importance.

Then, in the third decade of the century, appeared upon the scene the patriotic statesman, Count Stephen Szechenyi, the greatest Hungarian of his century. With untiring zeal, and enterprise, and self-sacrifice, he endeavoured to raise his countrymen to the level of the Western European nations. Education, manufacturing industry, agriculture, engineering enterprises, were fostered and developed, without political agitation in the ordinary sense. Szechenyi well knew that no real and permanent progress in any direction could be achieved without conciliating the autocracy in Vienna. Without the social reforms and successful enterprises of Szechenyi a Semmelweis would have been impossible. But in the course of a few years his schemes were arrested, and all real progress overwhelmed and ruined by the influence of the political agitator Kossuth and his colleagues and co-extremists. With eloquent tongue and ready pen he preached the French doctrine of "liberty, equality, fraternity." He had been warned many a time by Szechenyi that his methods of agitation would bring about a political revolution, and cause infinite suffering. But Kossuth went on with his dramatic agitation, sturdily enduring Austrian persecution, and apparently making progress. So Szechenyi gradually receded from popular favour, and although he took his share in the administration of affairs after the Revolution, he saw that genuine progress was doomed, and he ended his great career in despair and melancholia within a retreat in the same suburb of Vienna where the career of Semmelweis reached its tragic conclusion a few years later.

Meanwhile, Kossuth, Déak and others worked as if their whole aim and object in life was to bring on a political revolution; and it arrived just after that of Paris and Vienna in 1848.

The autocracy yielded with suspicious alacrity to the will of the democracy; every political advantage and privilege demanded by the revolutionists was conceded. The Hungarian people were triumphant, and Kossuth was universally proclaimed as a hero and the saviour of Society. The triumph was short-lived.

There can be little doubt that the Court and autocratic officialdom of Vienna stirred up the Croats, Serbs and Wallachs, and egged them on to attack the Hungarian people. This meant civil war, which afforded the pretext for sending Windischgrätz upon the congenial task of suppression. He arrived at Buda-Pesth with his military force early in 1849, and proceeded to settle the troubles by sterner measures than it had been considered politic to apply to Vienna the year before. The hastily-levied Hungarian armies were commanded by an inspired military leader, Görgei, and Hungary's right to nationality was vindicated by the military prowess of her sons. When they were victorious everywhere and the time had arrived for an advantageous and honourable compromise, Kossuth perpetrated the folly of persuading his parliament to proclaim the establishment of a republic and the deposition of the House of Hapsburg, in the person of the youthful Emperor Francis Joseph. But Kossuth was no Cromwell, and even Cromwell failed in the end. Then followed the Russian invasion and the overwelming of the Hungarian armies everywhere, the surrender at Világos in 1849, and the savage suppression by the atrocities superintended by Haynau. Kossuth and some colleagues sought liberty in Turkey, while many men of mark in Hungary, too proud to flee, remained to be captured and to take the consequences of their patriotic action. It was just one year before the return of Semmelweis that Count Batthyani was shot as a rebel at Buda-Pesth. With wounds so recent and so deep it may readily be imagined that Hungarian Society was too depressed and enfeebled for strenuous enterprises, either intellectual or commercial, in the year 1850. Such was the state of society to which Semmelweis voluntarily returned after his disappointment in Vienna in 1850.

The English medical reader, who may have neither the time nor the inclination to study formal historical works, can most readily obtain an accurate impression of the state of Hungarian Society at that time by reading the translations of the stories of Maurus Jókai, one of the literary men of genius of the revolution period.

When Semmelweis returned home he found a condition of things in his native city which might well have extinguished the ardour of the most enthusiastic devotee to the advancement of medical science. Buda-Pesth was under an absolute Government administered from Vienna: the city had been proclaimed as in a state of siege, and in that state it remained for another five or six years. Austrian spies and police agents swarmed everywhere creating work for themselves, exciting suspicion among people who wished to be friends, so that no man knew whom he could trust.

The Medical School had almost ceased to exist, and medical teaching and writing were entirely in abeyance. Many of the professors and students had, as in Vienna, Prague, Milan, Florence, taken part in the Revolution, and the suppression had been stern in the extreme. Some professors had been deposed, like Paul Bugát, the editor of the only medical journal which had been published in Hungary; the journal itself had ceased to appear having gone under during the political storms of 1848. Many of the members of the medical profession, if they were even suspected of sympathy with the agitation in the cause of freedom, were placed under police surveillance. v. Balassa, the professor of surgery, a man remarkably gifted and highly influential with his professional colleagues and with the public, "*die Zierde der Universität,*" was thrown into prison. Many others threatened with similar treatment had found liberty beyond the frontiers of the Austrian dominions.

The police spies affected to scent out political conspiracies even in the meetings of scientific associations. The Hungarian Academy of the Sciences (die ungarische Akademie der Wissenschaften) was compelled to suspend its sittings, and the Medical Society of Pesth was permitted to meet only under strict, even insulting conditions. At every meeting a police official attended to control the proceedings, and the minutes of the transactions had to be submitted to the authorities. (*Bruck,* 51.)

In every department of intellectual life including that of Medicine extreme stagnation prevailed. The activity and interest in more exact method of medical work, which v. Balassa had introduced from Vienna in the early forties, had become completely arrested, and that at a time when everywhere beyond the boundaries of Hungary a feverish activity had sprung up and a new life had awakened in every field of natural science. This was the time when the whole of Hungarian medical publication had contracted to the dimensions of the officially censored Minutes of the Pesth Medical Society.

Such was the state of affairs when Semmelweis returned apparently a broken man. He had gone forth in high hopes as a student of medicine leaving father and mother, brothers and sister behind him to watch his career: now on his return there was not a soul to welcome him: father and mother dead, brothers refugees as rebels; only one brother a minister of religion who had patriotically assumed a Hungarian name, and a married sister remained to him in all the world. A very few of his old Vienna associates, chief of whom was Markusovszky, were residing in Buda-Pesth, but it was not so easy to renew associations. There were plenty of intellectual men with active temperament eager to work and exchange views, but the political condition weighed like an incubus upon Society and made association in intellectual work almost impossible. Korányi* said in reminiscence of the period, "We never saw one another, no man could learn what scientific efforts the other was engaged upon: we lived as it were in the darkness of night."

Whatever may have been the intentions of Semmelweis when he left Vienna with regard to professional work and the spread of his principles, this depressing atmosphere of gloom and inaction appears to have completely closed in upon him, and brought on apathy and passivity. He made no effort to overcome his natural diffidence, and he accepted the alleged incapacity to find expression for his opinions in writing as a final conclusion not be revised or reviewed. Yet he had astonished his friends in Vienna by the admirable way in which he acquitted himself in the Medical Society meetings of 1850.

He had little or no professional work to do for a considerable time, and he might have written much, but he began no new literary work, and trusting to the spread of his *Lehre* by its own inherent merits, he left the field open to the attacks of his bitterest antagonists. His material resources were probably becoming narrow, and he had ere long to turn his attention seriously to practice in order to earn a living.

APPOINTMENT AT ST. ROCHUS HOSPITAL.

In spite of the "darkness of night" of police administration referred to by Korányi, there appears to have still remained some means of social intercourse and exchange of opinion among members of the medical profession in Buda-Pesth.

Semmelweis† says in one of his rare autobiographical passages: "One of my first evenings in Pesth I spent among a large company of medical men. Because of my presence the conversation turned upon puerperal fever, and an objection was raised to my opinion about the causation of the malady on the ground that at the St. Rochus Hospital of Pesth there was at that very time a severe epidemic of puerperal fever prevailing, and that such an occurrence was usual every year; yet students did not make examinations there with hands rendered unclean by decomposed animal matter, for in the maternity portion of the St. Rochus Hospital no teaching of midwifery was carried on.

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* Bruck, p. 52.

† *Ætiologie.*

"On the following morning I paid a visit to the hospital in order to obtain evidence for myself. I found there a woman who had just died of puerperal fever and whose body had not yet been removed, one was at the stage of agony, and four other lying-in women were severely affected with puerperal fever (schwer am Puerperalfieber erkrankt). The other patients in the room were not lying-in women at all, but sufferers from other diseases. Here then were the circumstances of an unfavourable environment for lying-in women definitely ascertained, but not in opposition to my opinions about the causation of puerperal fever; on the contrary, the surroundings and circumstances were entirely in harmony with my opinions."

Further information proved that the maternity portion of the hospital was not a separate and independent Division, but an annex to the Surgical Division. The Obstetric Primarius was consequently at the same time chief of the surgical staff. He was also in addition *Gerichts-Anatom,* that is, official maker of post-mortem examinations for legal investigations. Moreover, owing to the want of prosectors the autopsies had to be made by the various members of the staff for themselves.

The Senior Surgeon was accustomed first to make his visit to the surgical cases, and then proceed to the lying-in portion. Even though there were no students to examine the patients with hands rendered unclean by cadaveric poison, the surgical and medical members of the staff used to make examinations with their hands rendered unclean by manipulations upon surgical cases in the contiguous wards. The chief factor in the production of puerperal fever here was the decomposed animal matter of various kinds produced in the surgical division of the hospital, and conveyed by the hands of the staff to the lying-in patients. It should be remembered that in the times before Lister, suppuration, erysipelas, and even gangrene of wounds were of almost constant occurrence among patients in the surgical wards.

"St. Rochus Hospital is an infirmary belonging to the Commune of Pesth, and it contains 600 beds. Its staff consists of three medical and two surgical seniors: the lying-in portion belongs to the surgical side.

"During the months when the Obstetric Clinic of the Faculty of Medicine of the University of Pesth is open, maternity cases are not admitted into the St. Rochus hospital, in order that material for teaching may not be withdrawn from the Obstetric Clinic; only during the long vacation in August and September, during which time the University Obstetric Clinic is closed, are maternity cases admitted to the St. Rochus hospital. During the other ten months of the year the midwifery beds are occupied by surgical cases."

The causes of the yearly "epidemics" were now quite clear to Semmelweis, and he greatly wished for the opportunity of introducing changes in the arrangements which would put an end to the waste of lives, and produce a new and perfectly reliable demonstration of the correctness of his teaching as applied to practice.

So he addressed a request to the City authorities to give him the direction of the lying-in portion of the hospital and the opportunity of bringing the endemics to an end. His application was successful.

"On the 20th May, 1851, I took charge of the Obstetric Division of the St. Rochus Hospital as unpaid honorary senior physician, and I performed the duties of the office for six years." The connection between the surgical and obstetrical departments was severed: the beds for lying-in women during the vacation were devoted to gynæcological, not surgical, cases during the rest of the year; thus the chief cause of puerperal fever was removed, and it never again occurred to any considerable extent. Chlorine disinfection was introduced and everything was arranged just as in the First Obstetric Clinic of Vienna."

His results were wonderfully satisfactory. In the school-year vacation months from 1850–51 to 1855–56 inclusive, the number of patients delivered at the St. Rochus hospital was 933: of these 24 died, but only 8 from puerperal fever, that is a mortality of 0·85 per cent.

This new arrangement permitted Semmelweis to have charge of from 120 to 200 cases a year—a sad downcome from the First Obstetric Clinic of Vienna.

In addition to the phenomenally insanitary condition of his rooms, which looked out upon the cemetery, he met with heart-rending difficulties, owing to the untrustworthiness and want of conscientiousness on the part of the nursing staff and even of his colleagues. For example, an assistant surgeon went to take the management of a case of labour direct from making a post-mortem examination on a man who had died of gangrene, and the patient died of puerperal fever. But Semmelweis persevered and became watchful, and the epidemics disappeared as at a stroke. The mortality the first season fell to 0·85 per cent. It used to be fearful, according to all accounts, since the establishment of the anatomical basis in 1840, but the records had been lost during the Revolution, and the exact figures could never be obtained. The sudden change for the better brought Semmelweis a great reputation in Pesth, and he needed it as a help to practice. In his first year he had his right arm broken twice, once in the riding school and once in the swimming school, and he was for a long time disabled, and meanwhile his patrimony was running low. Still, he did not lose courage, and shewed the happy disposition with which he was endowed as if nothing had happened. He enjoyed existence, and appeared for the time to have thrown off the irritability and sensitiveness, which he had always exhibited in Vienna, with regard to the neglect of his *Lehre*. He found in Professor Birly, who, like Klein, had been an assistant to Boër, a strenuous opponent of his doctrine. Birly stuck to his theory that puerperal fever resulted from neglect of the *primae viae,* and believed the only efficacious treatment was the abundant use of purgatives. It is curious to reflect that a belief in this treatment was in all probability brought from England by Boër over sixty years before, where it was the ''sheet-anchor'' of the leading obstetricians of that time. Birly would not give the prophylactic disinfection even a trial, but Semmelweis had grown tolerant and had no quarrel with his elderly chief.

That Semmelweis did not feel at home during his first years in Pesth, and probably regretted his hasty abandonment of his career and desertion of his friends in Vienna, appears to be indicated by his frequent candidatures for chairs in various universities. One curious incident occurred when he was candidate for an appointment at Prague, when Lange was called to Heidelberg in 1851. He and Chiari and v. Arneth all travelled to Prague in the same train as candidates, and were all rejected on the alleged ground that they did not speak the Czech language. Under the successful candidate the mortality in the Midwives' School rose in a few years to 13 per cent.

During the years until the death of Professor Birly in 1855, Semmelweis devoted himself to his life-saving work at St. Rochus Hospital, and in trying to build up a practice, in which he was fairly successful. But he made the fatal mistake of acting in the belief that his Doctrine must make its way in the world by reason of its appeal on its merits to the teachers of midwifery. It was true, therefore, it must prevail, as Routh wrote to him in 1849. He never raised a finger on behalf of his own cause: never a line from him ever reached friend or enemy, supporter or opponent.

Professor of Midwifery in the University of Pesth.

Semmelweis was still leading his uneventful life of routine professional work, varied by routine social incidents, when Professor Birly died suddenly early in 1855.

An inspired article from Pesth appeared in the *Wiener medicinische Wochenschrift* which frankly indicates the situation thus created. "The late Professor of Midwifery was an honourable man whose sudden death has caused the deepest sympathy and regret in the widest social circles. He was a man of learning, and possessed all the endowments required in a teacher, and yet it would not be going too far if we expressed the opinion that long before his death he had ceased to be a professor of midwifery according to the present day requirements of medical science, teaching the manifold methods of diagnosis and the new ideas with regard to pathology so urgently demanded in every good school of medicine. . . . It is no indiscretion to mention that both professional and public opinion supports the appointment of Dr. Semmelweis to the vacant professorship. . . . . Dr. Semmelweis, when assistant in the Lying-in Hospital of Vienna acquired, owing to his lectures and his courses of practical and operative obstetrics, a reputation extending far beyond the boundaries of the Monarchy; and he has attained already a great position in medical practice in our city. . . . If the recently revived scheme of erecting a new lying-in hospital is carried out, and proper facilities for teaching are afforded in it, then will be opened up to our energetic obstetric specialist a wide field of activity and a new era in obstetric science will commence in our Father Land."

Here was a plain hint to the city and to the Government authorities, but it could scarcely have been required. It would have been too offensive a "job" for the Minister of Education to have passed over Semmelweis. He had been on his good behaviour for about seven years: he would be known to the Austrian authorities as a patriotic but no longer a political Hungarian citizen; times of milder and less meddlesome government had arrived, and it would have been contrary to the prevailing policy of conciliation to give offence to Hungarian sentiment; besides Semmelweis had no rival worthy of consideration.

So in July, 1855, Semmelweis was appointed Professor of Theoretical and Practical Midwifery in the University of Pesth.

This appointment crowned his ambition, and it seemed to work a transformation in his character. His ambition flamed up once more, and the old energy and enthusiasm of the Assistantship period in Vienna revived. He had a trying task before him; the means of teaching both at the University and the Hospital had become derelict by the neglect of years, and it required all the energy and enthusiastic devotion of even Semmelweis to create something like a modified efficiency.

The Obstetric Clinic.

The Clinic was vastly too small and it was unfavourably situated. Owing to the fewness of beds and the cramped space he could only admit patients who were actually in labour, and they had to be hurried out on the ninth day or earlier. Once more his patients' view from the windows was the burying ground, varied on the other side by glimpses of the dissecting room with underneath the privies and an open sewer. During the first summer session he had 93 midwives and 27 medical students in his class, and the wards were so small that there was hardly standing room for the students who overflowed into the whole of them from the labour ward when an operation was proceeding. There was no lecture theatre, and Semmelweis had to teach in the corridors or on sufferance in any classroom available.

But there were other than constructional and physical difficulties. The whole lying-in hospital staff were unfriendly and even disloyal. They had never been trained to cleanliness and some of them would not be taught by the faddist who had been thrust upon them as Director. Opposition everywhere and in everything. The governing body's representative was so economical that he would not provide sufficient stores, not even bedclothes and "washing." Semmelweis was not discouraged; he became more determined than ever to succeed. Only the need for watchfulness put a great strain upon him in his efforts to improve the professional work of the hospital. His biographers nearly all mention the fact that impatient of the delay of the authorities in supplying him with linen, he went and bought a supply himself and he had the greatest difficulty in obtaining repayment.

But his own self-sacrifice, and his exacting firmness of behaviour towards his staff, and his foresight had the most excellent results as far as the patients were concerned: in the first school year there were 514 cases, and only 2 patients died of puerperal fever, that is only 0·39 per cent., an unprecedented result in that institution.

Semmelweis indicated some revival of a desire to spread his principles by contributing a report of the lying-in hospital for the year 1855–56 to the *Wiener medizinische Wochenschrift* in which he attributed his success to the exact application of his own method of chloride of lime disinfection. How little progress the *Lehre* had made, when relying on its own merits without an advocate, is well indicated in an editorial note following the communication of Semmelweis: "We thought that this theory of chlorine disinfection had died out long ago: the experience and the statistical evidence of most of the lying-in institutions protest against the opinions expressed in this article: it would be well that our readers should not allow themselves to be misled by this theory at the present time."

Such is the privilege of the anonymous medical journalist and reviewer—to exhibit ignorance not without personal animus in assailing the exposed victim from the safe shelter of anonymity.

In 1856 Professor Klein died, and Carl Braun, who had succeeded Semmelweis as Assistant and had been permitted to retain that position for five years, only resigning to accept a call to a professor's chair in Trient, was appointed successor.

It would be useless to speculate on what were the feelings of Semmelweis over this event. What hopes and aspirations he may have entertained about the influence of his principles, and their possible fruition to the extent of recalling him to Vienna. But he naturally did not see things in due perspective: whatever influence his old friends in Vienna might have been able to exercise in his favour—and they were all there still,—they were not likely to move after their experience of his behaviour in 1850. Then he had written and published nothing. For Englishmen it is difficult to appreciate the importance of "original work" upon professional promotion in Germany. It is the cause of the production of the floods of long, dreary, unreadable contributions, which swamp their professional journalism. With this German usage we have no quarrel: we are not compelled to read all: we are at liberty to make more or less judicious selections. But Semmelweis had done nothing to attract attention, he had published no work; he had so exaggerated in his own mind the importance of his principles that he believed due recognition was certain to come; and he had now the bitter experience of seeing Carl Braun Professor and Director of the First Clinic. Here then was another antagonist in the Chair of Obstetrics in succession, but an antagonist of a very different type: not merely a silent, resentful and jealous man, like Professor Klein, but a master of gibe and sarcasm, and not too exacting with himself in the matter of strict scientific and historic accuracy. His methods of controversy conformed more nearly to what we recognise as the political method of a private Member of Parliament, than fitted for the clear cold air of impersonal scientific discussion.

After recovering from the depression produced by the deep wound of disappointed ambition, Semmelweis appears to have resolved to prove to the whole world that the application of his method could produce the best results ever known in the history of obstetrics. His own Clinic at Pesth would put the Vienna Lying-in Hospital to shame, and humiliate the rival who had been promoted in preference to himself.

If such thoughts were in his mind, he was soon to pass through a painful and disappointing experience. In the course of the school-year, 1856–57 he lost no fewer than 16 patients from puerperal fever. What could be the cause of this disaster? He at once perceived what most men with less clear insight and more limited experience would have overlooked: the new-born infants of the women who were attacked with the disease remained well. This was in striking contrast to the effects of tedious labour observed in Vienna. Hence the infection did not occur before or during labour: the infection was therefore conveyed to the patient in the lying-in state, and inasmuch as the patients were not examined as puerperæ unless they became feverish, some other external agent must be at work. That operative external agent must be discovered. Semmelweis therefore paid strict attention to the details of nursing as practised by the midwives and pupils: he examined the instruments, the utensils and the bedding; and he made some shocking discoveries. He found that the patients in labour were laid upon filthy sheets which actually stank of decomposed blood and lochia. These had been received and accepted as clean by the head-nurse from the laundry contractor, who had accepted the contract at a specially low rate. All the circumstances pointed to corrupt practices from the superintendent to the pupil-midwife. So, with his whole heart and soul filled to overflowing with his aspirations to save from suffering and death the poor creatures consigned to his care, Semmelweis had to look after the washing! Meeting with official apathy and procrastination while the unhappy women were perishing, the energetic Director of the Lying-in Hospital one day bundled together some of the evil-smelling "Wasche" just as they were taken from the beds of new patients and went straight to the chief official person, von Tandler by name, and demonstrated the urgent call for improvement to his eyes and nostrils. He obtained all that he demanded for the comfort and safety of his patients, and that in an astonishingly short time, but he had earned the dislike and hostility of the head of the hospital administration, whom he had made to appear ridiculous.

Among the causes of opposition to Semmelweis and resistance to his efforts at amelioration was one to his infinite credit: he thought first and chiefly of the welfare of his patients; all else was subsidiary. He did not spare himself, and he expected his staff from highest to lowest to shew the same devotion to duty. That was too much to expect of the lazy, undisciplined crowd who had become demoralised under the formal directorship of Professor Birly. His prophylactic disinfection was carried out carelessly and apathetically except under his own eyes, and his disloyal staff were quietly encouraged by the narrower-minded and jealous among the midwifery practitioners in the city. It was inevitable that a man who had acquired the fame of Semmelweis must be envied and thwarted by the baser sort of neighbours and "colleagues." The result of this opposition and disloyalty was a temporary increase in the mortality from puerperal fever within the lying-in hospital: but happier times were soon to dawn upon the pioneer.

The Medical Journal.

The state of siege under which Buda-Pesth had lain since the suppression of the revolution of 1848 had now been raised in 1854, and the amnesty had been declared in 1856. In the course of the last few years a process of reconstruction and some revival in the arts of peace had been developing in Hungary, and one symptom of the change was an interest in medical science and literature. The recognised leader of this movement in Buda-Pesth was von Balassa, Professor of Surgery, who had spent a considerable time in prison on account of the frank expression of his political opinions at the Revolution time. He was supported in his proposals by many men of mark in the medical profession in Hungary, especially by all that was young and promising. Among these were Semmelweis, Markusovszky and Hirschler. The Medical Society of Buda-Pesth, for long in a languid condition, was revived and stimulated into activity: and under the same leadership and influences chiefly that of v. Balassa, a medical journal was started and established, the "Orvosi Hetilap" or Medical Weekly, Wochenblatt or Semaine médicale. The first number appeared in June, 1857, under the editorship of Markusovszky. The foundation of a medical book-printing and publishing company was soon to follow. Thus were laid the foundations of the various successful medical reforms and enterprises which have in the course of half-a-century placed Buda-Pesth in the position of one of the first centres of medical education in Europe.

Wherever in a centre of population a medical journal is established even for a time, even when the enterprise is not permanently successful, there the interest in professional work in its scientific aspects increases and the standard of professional efficiency rises. So it was in Buda-Pesth where a certain intellectual resiliency was just returning after the ponderous weight of bureaucratic and police interference was beginning to be alleviated. We can imagine the meetings and consultations, the comparing of notes and the contributions of suggestions and ideas to the scheme by all the members of the medical profession, residing in Buda-Pesth and other centres of population, who were the ablest and most patriotic. The founding of the medical journal *Orvosi hètilap* was an event of primary importance in the life of Semmelweis; it taught him at last that he could give expression to his thoughts and feelings in literary form, and it kept him in close association with the aristocracy of Hungarian medicine, just as he had been privileged to enjoy the society and friendship of the best and ablest teachers in the Vienna School of Medicine.

Semmelweis, as might have been expected from his temperament, was carried away by the new movement. He read contributions to obstetrics and gynæcology in the Medical Society, and became a strong official supporter of the medical journal both by written articles and contribution to the management. The "congenital aversion to all that is called writing" disappeared, and in time he went perhaps somewhat to the other extreme, for in his last years the pen was seldom for long out of his hand. His "Doctrine" was ever present in his mind, and he joined in the battle against the ancient dogmas "in the name of humanity and for the advancement of medical science."

There can be little doubt that for several of the first years of his life in Buda-Pesth Semmelweis felt that he was an exile from Vienna, and constantly entertained the hope of return to the larger life and the unbounded opportunities of professional work in the centre of the Austro-Hungarian Empire; such, at least, is the opinion of Bruck, who was among the best biographers. But he ultimately settled down to the life of a patriotic Hungarian citizen, and devotion to his professional and social duties. Nothing could indicate this change in a more striking and specific manner than his unhesitating refusal of a flattering invitation to accept the appointment of Professor of Obstetrics and Gynæcology at the University of Zürich. It is a curious coincidence that his bitterest opponent, Carl Braun, received a similar 'call' to Zürich two or three years before, just about the time when the Chair of Midwifery at Vienna became vacant by the death of Professor Klein.

MARRIAGE.

In June, 1857, Semmelweis, now fully 38 years old, married a charming young woman of 18, Marie Weidenhofer by name, evidently of a family belonging to the old German colony.

The appearance and character of Semmelweis at this time were described and lovingly dwelt upon long after his death by his widow and personal friends. He had become rather bald and inclined somewhat to corpulence. He was well set up, and of a florid and healthy appearance. Every word and action expressed unbounded kindness of heart. Towards the poor and suffering he was always kind and helpful. He was strict and exacting both towards himself and others in professional matters; and he was inexorable in his demands for exactitude in carrying out his prescriptions with regard to disinfection. He was readily excited and hasty in temper, but easily soothed and calmed down.

In his professional work he was extremely conscientious and devoted to duty, responding promptly to every call for his professional services from rich or poor by day or night. Probably he gave little thought to professional fame and emoluments. He was a benevolent and skilful physician, but not much of a business man. He knew nothing of pushfulness, and held in contempt the little artifices by which so many of his neighbours sought to attain to rapid success. His want of conventional airs and graces did not ingratiate him with the ladies of the upper circles of Buda-Pesth, and he was too frank and honest for the highest success in his speciality as measured by the lay mind. "Countess, I congratulate you; I have made a mistake," he exclaimed on one occasion to a patient when he found he had made a wrong diagnosis. He had mistaken a benign pelvic tumour for cancer!

Towards the end of 1858 the first child was born and died within forty-eight hours from hydrocephalus. In November, 1859, a second child arrived but this one did not long survive; after four months of very feeble health the little daughter was also carried off from peritonitis. So after four years of married life the parents were still alone. The mother by all accounts was a healthy woman and lived a long and fairly vigorous life; it would seem probable, therefore, that Semmelweis himself must have had some weakness of constitution, most likely tubercular, which may be surmised to have brought about certain changes in his brain not conducive to longevity.

The domestic life of the married pair was simplicity itself—filled with the chronicle of small social incidents arising out of kindly hospitalities accepted and returned, chiefly in medical circles. But his professional duties and relationships brought him much vexation of spirit for still a long time, if indeed they ever settled down to the average inherent in the situation.

HOSPITAL WORK.

The routine work consisted in a visit to the hospital at seven in the morning, followed by a lecture wherever he could find accommodation. The primitive state of the medical school of those days is well indicated by the fact that the lectures were addressed to both medical students and midwife-pupils, about 120 of them altogether, sitting or standing where they could. The students of medicine were not much interested in midwifery because it was not a compulsory subject of examination for the doctor's diploma, and the midwife-pupils were too illiterate to understand the lecturer however elementary the instruction was made. Another difficulty was that Semmelweis preferred to speak in the Hungarian language in which he had comparatively little practice since his boyhood. Besides the Hungarian language did not as yet contain the necessary technical terms to enable him to maintain the dignified style expected of him, even if he had had a full command of the language. He had the option of lecturing in the German language, but the midwife-pupils would have some difficulty in understanding it; and then there is just a suspicion that Semmelweis was influenced to some extent by a patriotic sentiment in favour of the mother-tongue.

Sometimes when the female pupils exhibited stupidity or incapacity for sustained attention, Semmelweis became sarcastic and scolded, the medical students laughed, and then the female portion of the audience wept and sobbed. No progress could be made until some small jokes and a little banter had soothed the injured susceptibilities of the women students.

But the rigid application of his method for the safety of his patients was no joking matter. His strictness as an examiner brought upon him the hatred of rejected candidates and their desire for revenge, and to such a pass did matters come that his wife sometimes feared for his life. All that the envy and hostility of rival practitioners could do to thwart and annoy him was accomplished, and Semmelweis, always reticent about professional affairs with his wife, could only give vent to his anger and disgust by an occasional private outburst among his more intimate friends and colleagues. (v. Waldheim, p. 126.)

The school-year 1857–58 caused Semmelweis much distress and disappointment. The results were again unfavourable. Out of 449 women confined in the Lying-in Hospital 18 died of puerperal fever, that is, 4 per cent. So he went to work to discover the cause, and he soon learned that a nurse trained to his own methods had so little conscience that she did not change the stained and foul-smelling sheets in the bed in which a patient had died from puerperal fever : and this neglect was habitual. There was no longer any excuse or palliation on account of a deficient supply of linen and of scamped washing : the neglect appeared to be merely the expression of laziness, apathy or sympathy with the disloyal juniors who jeered at the eccentricities of the Professor.

Another illustration of the difficulties so gratuitously thrust upon him is mentioned by Semmelweis himself in the *Ætiologie* (p. 101). Two cases of gangrene of the external genitals, as seen in lying-in women, occurred about the same time in the Lying-in Hospital. There was no possibility of isolation: the space was too limited. As the next best plan, Semmelweis told off two midwife-pupils as nurses, each to attend to one case: they received instructions not on any account to touch any other case. In spite of these explicit orders one of the pupil nurses was caught in the act of examining a patient who was in labour immediately after her admission into the hospital.

It was not to be expected that Semmelweis could with impunity offend the dignity of such a high and mighty official as Statthaltereirath von Tandler, under whose nose he had thrust the vile-smelling napkins snatched from the bed of a lying-in patient. Ever since that absurd incident von Tandler appears to have taken a special interest in the Lying-in Hospital and its Director, doubtless with the object of tripping him up if an opportunity occurred. Von Tandler appears to have encouraged the younger or subaltern officials who had access to the Clinic to bring him confidential reports about what went on there, in fact to spy upon Semmelweis and the head-midwife who was loyal to her Chief.

When therefore in the school-year 1857–58, in spite of the lavish outlay in providing new furnishings, the mortality from puerperal fever again increased, one of the underlings, who could have no personal grudge against Semmelweis or the head-midwife, delighted the *Statthaltereirath* by bringing some sensational intelligence. He reported that early in 1858 ten lying-in women died at the Clinic in one day, and that this tragic incident was entirely owing to the carelessness of the head-midwife. It was alleged that this head-midwife permitted bed-clothes soaked with blood and discharges from the bed in which a patient had just died to be spread under a women in labour immediately after admission, and that the head-midwife was nevertheless allowed to retain her position, although it would be remembered that a nurse (Wärterin) convicted of similar malpractices had been dismissed on the spot. This talebearing appeared to afford von Tandler the opportunity for which he was waiting. So without seeking corroborative evidence he proceeded to humiliate the Professor of Midwifery by a formal official admonition : "Confidential communications have been received concerning several unfortunate occurrences and abuses at the Obstetric Clinic of the Imperial Royal University. It is alleged, for example, that through the carelessness of the head-midwife the bedding of lying-in women is not only very seldom changed, but that bed-clothes soaked with blood from dead puerperæ are put under patients who have just been admitted, and that in consequence of all this the mortality at the beginning of this current year had reached such a high degree that ten lying-in women died in one day. This fact is all the more remarkable inasmuch as last year with a much smaller mortality the Herr Professor applied for and obtained a large grant of money in order to enable him to fulfil all his requirements with regard to bed-linen, and all this was done at such lavish expense that the high expenditure did not escape the attention of even 'the high Ministry for Culture and Education.' The Herr Imperial Royal Professor appears therefore to share the opinion of other people who have visited the Clinic, that the blame for the increased amount of sickness and mortality should not be thrown upon the want of 'washing,' nor upon the irregularity of its delivery on the part of the laundry women, but upon the carelessness and irregularity with which the bed-linen is changed by the nursing staff."

To this humiliating reprimand Semmelweis solemnly replied at considerable length (*Ætiologie,* p. 96). According to his usual exact manner he began by quoting the terms of the official document, and admitting that there was no longer any deficiency in the quantity of "washing," but there had been grave irregularities with regard to the changing of the bedding, and to these must be attributed the increase in the morbidity and mortality in the Lying-in Hospital. But for these irregularities it was not the head-midwife who was to blame, but the Wärterin N.N., who had been dismissed in consequence.

"In the school-year 1856–57 there were 16 deaths from puerperal fever owing to deficiency in the quantity and careless and irregularity in the delivery of the articles. . . . No more than two patients died on any one day: and if it is alleged that at the beginning of 1857–58 such a high degree of mortality prevailed that ten women died on one day the statement does not accord with the facts. . . .

"From the time of Hippocrates until very recent years it was the indisputed opinion of the medical profession that the frightful devastation wrought upon puerperal women must be ascribed to epidemic, that is to say, to atmospheric influences. These influences, being beyond the control of medical science, expressed themselves in destruction of life without let or hindrance. It was I who in the year 1847 at the great Lying-in Hospital of Vienna succeeded in proving that these old opinions were false, and that every case of puerperal fever was the result of infection. Owing to the measures which I employed there has been no epidemic at any of the three lying-in institutions with which I have been connected, and these used to be visited every year by frightful epidemics. . . .

"My discovery of the cause of puerperal fever has been compared in its beneficial results to the introduction of vaccination for the prevention of small-pox by Jenner. I am painfully conscious how unbecoming it is of me to say such things myself, but the fact that my Clinic has been aspersed by allegations about a high death-rate compels me to speak in my own defence. It must place the mortality at the Obstetric Clinic of Buda-Pesth which has occurred without fault of mine after nine years of brilliant success in a more favourable light."

The result of this rather undignified dispute was entirely satisfactory to Semmelweis. By the dismissal of the head-nurse and the thorough attention to cleanliness in every respect which followed the plague was stayed. The outbreak of puerperal fever during these three years was naturally no disproof of the efficacy of chlorine disinfection. As Semmelweis here points out, it could have no effect upon soiled bed-linen: it could only free the well-washed hands of deleterious material.

It should be kept in mind when estimating the part which Semmelweis had to take in this unhappy discussion that he was defending himself from an attack by a paltry official person who, by a strained use of his authority, might have driven him from his chair. It was not so many years ago since Hungarian professors had been cashiered by the half-dozen. Semmelweis also knew that about him was a group of small envious men in his own special department of medicine who were ravening for promotion, and were willing to obtain it by the arts and principles of Sir Pertinax McSycophant and Judas Iscariot. Of such a type unfortunately the supply has never failed in all generations, and in all centres of medical organisations and rivalry. It was by such arts that Boër, the greatest man whom they had ever seen in Vienna, was deprived of his professorship. No pity was shown to the sad and lonely old man by the harpies who set upon him. Semmelweis did well to act cautiously for once.

Then apart from personal considerations he was desperately in earnest in safeguarding the lives of his patients, and the unfortunate results of the last three years was seriously obstructing the adoption of his prophylactic methods. There was even indecent jubilation among his more bitter antagonists at his failure and humiliation, while many of the more thoughtful and sober-minded men in official positions were made to doubt and hesitate. Meanwhile poor lying-in women were perishing throughout Europe as if Semmelweis had never spoken. But the physical difficulties with which the professor of Midwifery had to contend were also discouraging in the extreme.

On account of the insanitary condition of the Obstetric Clinic and defects in other departments of the Faculty of Medicine of the University of Buda-Pesth, the whole college or Committee of professors (Professoren-Kollegium) had made urgent appeals for amelioration. The last time was in 1855; nothing had been done. Now after another three years Semmelweis addressed an urgent (Gesuch) request to his professorial colleagues and to *hohe k. k. Statthaltereiabtheilung* in Buda-Pesth, the department which had control of the hospitals, for permission to leave the extremely insanitary and insufficient portion of the hospital assigned to him. In this *Gesuch* he gives a description of the Obstetric Clinic which is worth quoting as a record of the degradation to which indifference to the welfare of the people of Hungary by Austrian bureaucrats had permitted a lying-in hospital to descend only fifty years ago. The Clinic contains 26 beds for lying-in women, but the air-space should be two and a half times more than is actually allowed in order to comply with the Ordinances, and there is no consideration shewn for the crowd of medical students and midwives. Three of the wards are so small that they cannot contain half of the students, and though there are two rooms sufficiently large to contain them without packing so that they cannot move, the atmosphere becomes so vitiated during clinical lectures as to be injurious to the patients.

The chimneys from the chemical laboratory underneath form the sides of the windows in two of the lying-in rooms, and consequently the temperature in these rooms is at times intolerably high.

The space assigned to the midwifery department is so circumscribed that no room can be reserved for isolation of the infected, and the sick must therefore be distributed among the healthy puerperæ. Consequently puerperal fever is spread even though it is not a contagious disease in the ordinary sense.

There are two narrow courts, mere well-holes for admitting the light, on two sides of the Midwifery Clinic; in one of these courts on the ground floor, and on the first and second floors are the privies of the whole building. There is an open sewer on the level of the ground, from which arises a penetrating stench (*einen penetrirenden Gestank*). The ground floor of the building is occupied by the department of elementary and pathological anatomy, and immediately under the windows of the maternity portion is situated the outflow from it by which fluid matter from the dissecting room is poured out. At one side there is a well-hole for lighting of which the lumen is partly obstructed by the deadhouse for the whole of the Clinics. . . .

The only alternatives left to the professor of midwifery are either hermetically to close the windows and so permit the patients to suffer from vitiated air rendered still worse by the great crowd of students, or by opening the windows to admit to the patients air loaded with decomposed animal organic matter, by both the well-holes with the light.

. . . The fact that cases of puerperal fever occur more frequently in the lying-in hospitals than among the population outside is known not only to the medical profession but also to the laity, and in official documents the lying-in hospital has been called a "murder-hole" even by the non-medical officers themselves. Considering the fact that puerperal fever rages in the lying-in hospital the question has been repeatedly raised whether it would not be more humane to close the hospital altogether.

Only a dreadful dilemma has saved lying-in hospitals from destruction. It is certain that a large number of the patients in lying-in hospitals are carried off in the bloom of life by puerperal fever. Without lying-in hospitals a larger number of women would remain well, but then would begin the anxiety with regard to the maintenance of themselves and their infants, and as a consequence of their necessity, the result would be crime, abortion, child exposure and desertion, and child murder.

Semmelweis continuing maintains that he discovered the cause of puerperal fever, and had taught how to prevent the fatal malady, but owing to recent experiences his opponents are strengthened in their opposition and his supporters are made to doubt and hesitate. "Hence it arises that the spread of the *Lehre* is hindered, and the human race must suffer from the pestilence for a longer time than would have been the case if the success at the Obstetric Clinic at Buda-Pesth could have borne stronger witness to the truth of the Doctrine."

And he does not stand alone in condemning the unhealthy condition of the lying-in Clinic; all his present colleagues of the medical faculty had made representations, even before he became professor, concerning the injury done to the medical school by the state of the hospital. Then to show that such opinions are not confined to Buda-Pesth, he quoted a long, evidently inspired article, from a Vienna medical journal in which an account is given of the deplorable condition of the Midwifery Clinic and the miserable shifts to which the professor is reduced both in the care of the patients and in his teaching. Among the important defects to which attention is called in the article quoted is the entire absence of instruction in gynæcology. Students see nothing but medical treatment of surgical gynæcological cases, and are sent away to commence practice without the slightest knowledge of diseases of women.

For a few years a better state of affairs had prevailed and Semmelweis, while holding the appointment at the hospital of St. Roche, was able to give some clinical instruction in gynæcology. But he had been deprived of that appointment by the application of the same principles by envious neighbours which had been brought into play in order to embroil him with the University authorities and deprive him of his professorship.

In spite of all the appeals to human sympathy and to the ordinary commercial interests of the Medical School of the University, nothing was done to improve the space and accommodation of the lying-in portion of the hospital till 1860, and what was done even then was only partial and unsatisfactory.

But notwithstanding all the perturbations and annoyances arising out of his official relationships as Professor of Midwifery and Director of the Lying-in Hospital, Semmelweis was spending his days in quiet domestic comfort and happiness, and in strenuous professional work. He was watching with pathetic attention and wistfulness the progress of his doctrine in Europe, and although he was occasionally cheered by the publication of some article supporting his opinion, he had upon the whole to confess to himself the deepest disappointment. Everywhere except in Great Britain and Ireland he saw evidence of the unfortunate mistaken belief that he had declared cadaveric poison was the only cause of puerperal fever.

SPREAD OF THE DOCTRINE BEFORE DIE ÆTIOLOGIE.

Semmelweis had clearly expounded his complete doctrine in 1850 at the meetings of the Vienna Medical Society, but only the minutes of the transactions had been published to the world. His own negligence was the chief cause of his failure, but not the only cause. Professorial teachers of midwifery ought to have known better. Was it really ignorance, or was there an element of personal hostility obstructing the acceptance and spread of the *Lehre*? Semmelweis in his simple-mindedness never seemed from first to last to be conscious of the tone of arrogance assumed by the professors towards the young Hungarian assistant, and we shall see later that he was never influenced in his language or actions by any sense of reverence for the professors. What indeed could he feel but the contempt which he ultimately expressed in the Letters"? Unconscious of the grave responsibility resting upon them, professors of midwifery had accepted opinions about the Semmelweis doctrine at second-hand. Hebra and Skoda had written and spoken, and considerable publicity had been given to the addresses by Semmelweis himself in 1850. Still, instead of going to the original sources, many professors and others joined in the attacks upon Semmelweis without even clearly understanding what had been taught and consequently what they were denouncing! Misrepresentation had got a start and we shall find that it was never overtaken and annihilated even by the publication of *Die Ætiologie.*

One of the cheering incidents was the publication in the *Dublin Quarterly Journal of Medical Science* of August, 1857, of Dr. Edward W. Murphy's article entitled: "What is Puerperal Fever?" But this was only a break in the almost universal antagonism by the most prominent teachers in Germany. In 1857 appeared an account of a puerperal fever outbreak in the new Lying-in Hospital of Munich, from which it was evident that Professor Anselm Martin had learned nothing from Semmelweis. Veit, of Rostock, wrote against the doctrine in the same year, and Späth of the Second Clinic and Carl Braun, the successor of Professor Klein of Vienna, joined the chorus in opposition. Carl Braun, in the course of his unscrupulous attack, of which we shall hear more in the sequel, actually committed himself to the statement: "In Germany, France and England this hypothesis of cadaveric infection has been up to the most recent time almost unanimously rejected." Untruthful, spiteful, intended to wound, was the language of Carl Braun, but it had a wonderful satisfactory result: Semmelweis at last yielded to the entreaties of his friends, and resolved to begin the task for which he had hitherto declared himself unfit. Although Semmelweis had been sadly negligent of his duty in leaving the field of controversy for the last seven years to such antagonists as Scanzoni and Carl Braun, a certain amount of publicity had been given to the *Lehre* by the Vienna medical press, and the subject of puerperal fever was receiving more attention and discussion than ever before in Europe. Still there was going on the same wanton sacrifice of human life and the same infliction of miserable physical disabilities, owing to the general adherence of professors of midwifery to the ancient formulæ, the traditional irrational doctrines.

There was only one man living to whom the whole subject lay in the clearest light. By a flash of insight the discovery had come to him ten years before: experience and reflection had only strengthened his convictions and given them completer form in his mind. His friends kept pressing him to write: the book was, as it were, standing ready-made in his mind: it was his duty to suffering humanity to publish his doctrine to the whole world in order to put an end to the well-nigh universal manslaughter. To the appeals of his friend Markusovszky in particular he had always replied: "I cannot write." Now the detested rival Carl Braun had written that the doctrine of cadaveric infection had been almost unanimously rejected in Germany, France and England, the doctrine which he had expected to triumph because of its inherent truth. Then once more the eternally reiterated error that he had declared cadaveric poison to be the only cause of puerperal fever. Was it not under all these circumstances a crime to remain silent?

We have seen that Semmelweis repeatedly missed his opportunities of proclaiming the Doctrine when he alone was in a position to fully and clearly expound it.

Hebra had led the way in 1847, and Semmelweis was content to further spread abroad the discovery by means of private letters and the verbal communications of his personal friends.

Skoda, impressed with the immeasurable importance of the new idea, had given an address to the Vienna Academy in 1849, and used all his personal and professional influence in proclaiming the truth.

Semmelweis himself had been prevailed upon to address the Medical Society in 1850, and received such a reception that he might well have taken courage to publish at that time the equivalent of *Die Ætiologie* instead of ten years later, but he was content to permit even the matter of his addresses to lie buried in the minutes of the Medical Society transactions.

After his return to Buda-Pesth he might have been excused if he had craved for months of reposefulness and peace of mind after his Vienna experience. But he permitted himself to "stagnate in the weeds of sloth" for five years as far as concerned professional writing in defence of his Doctrine, and left the field open for the opponents to work their ill-will both with personal reputation and Doctrine. The "Pester Narr" became the subject of derision in the obstetric clinics of Vienna, and the Discovery was held up to ridicule in most of the lying-in hospitals of Austria and Germany.

The more active antagonists took full advantage of the absence of opposition and the open field. We now come to record chronologically as concisely as we can the spread of the Doctrine in Europe of which the chief agent was at first the publication of adverse criticism. With this object considerable liberty must be taken with the order of Semmelweis in the *Stimme,* etc.

We have seen how Wieger, immediately after his return to Strassburg from Vienna, endeavoured to interest French accoucheurs in the Semmelweis prophylaxis, and failed.

The next to make the attempt was Dr. F. H. Arneth, who began his *Wanderjahr* on a journey through France and Great Britain and Ireland the month after Semmelweis left Vienna. He found the students of Dubois' Clinic working at anatomy as well as midwifery without practising disinfection in any way. The mortality and puerperal illness were appalling. So Arneth sought and obtained the opportunity of bringing the Semmelweis Doctrine before the *Académie de Médecine* in January, 1851. The *Académie* appointed a committee to investigate the question, but Arneth never received any report of the finding. This committee appeared to treat Arneth with discourtesy and the subject with contempt; but Carl Braun is quoted by Semmelweis[1] himself as having been placed in a position to declare: "The Académie de Médecine of Paris under Orfila as president, in the year 1851, pronounced its decision against the theory of cadaveric infection. They also stated that the circumstances of midwifery practice at the *Maternité* and the *Clinique* were much the same as in the two divisions of the Lying-in Hospital of Vienna, and in both severe epidemics of puerperal fever occurred. They also expressed their opinion that chlorinated lime possessed no qualities to cause destruction of cadaveric molecules."

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[1] Aetiologie, 456.

The statements made regarding the similarity of the hospitals of Paris and Vienna appeared to Semmelweis not to be founded on any real knowledge of the facts, and he would not allow them to pass unchallenged. He says: "In Vienna the pupil-midwives have not only nothing to do with dead bodies, they do not even come into contact with the sick puerperæ. When the professor visits the puerperal fever cases he is accompanied only by the assistant and the *Institutsmadame*."

The condition of the Paris Maternité described by Osiander was objected to by some on the ground that it was too antiquated, but Le Fort, who visited the Maternité in 1864 says of it[2]: "The Maternité of Paris which I visited in the end of 1864 showed a condition of things which explained only too well its excessive mortality.

"The principal ward contained a large number of beds placed in alcoves like English horse-stalls along each side. Ventilation was almost impossible. Floors and partitions were washed perhaps once a month . . . the ceilings showed that they had not been white-washed for many a long year (depuis de longues années). Lying-in women who became ill were transferred to an isolation room regardless of the nature of the illness—puerperal fever cases and patients affected with diarrhœa, bronchitis, measles or any other eruptive fever. Midwife pupils attend normal lying-in patients and fever cases alike, and perform all the necessary manipulations for every class of case."

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[2] Des Maternités, p. 123.

Le Fort speaks of a general aversion to water at the Maternité: of the clouds of dust raised by sweeping the long-unwashed floors, and he goes on to say: "It is not astonishing that the Maternité of Paris has furnished a mortality without example in any European country. From 1860 to 1864 the patients numbered 9,886, of whom 1,226 died, equal to a mortality of 12·4 per cent."

Well might Semmelweis remark on the finding of the Paris Academy of Medicine[1]: "They are in error if they believe the conditions in the Paris and the Vienna schools for midwives are the same. With regard to puerperal fever there is an essential difference in the circumstances of the two schools and the resulting health conditions are essentially different."

"In Dubois' Clinic and the First Obstetric Clinic of Vienna the circumstances bearing on the production of puerperal fever are identical and consequently the mortality is identical."

THE DOCTRINE IN GREAT BRITAIN.

The Doctrine of Semmelweis met with a very different reception in Great Britain and Ireland. The attitude which the leaders of the profession at once assumed towards the Discovery conferred distinction upon British Obstetrics. We have seen how Routh's paper on the "Causes of the endemic Puerperal Fever of Vienna," read in 1848, attracted great and promptly favourable attention from the leading teachers and writers on Midwifery in London.

Simpson, of Edinburgh, who had misunderstood the import of Arneth's letter in 1847, discovered his mistake and he had the quite unusual courage and candour to admit it. In the end of 1850 he contributed to the Edinburgh *Monthly Journal of Med. Science* an article expounding the opinions which he had then formed.

Simpson's article was entitled: "Some Notes on the

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[1] Aetiologie, p. 459.

Analogy between Surgical Fever and Puerperal Fever."

According to this article there is an analogy in the anatomical conditions of the subjects of puerperal and surgical fever. Patients during labour may be locally inoculated with a *materies morbi* capable of exciting puerperal fever, and this materies is liable to be inoculated into the distended and abraded lining membrane of the maternal passages during delivery by the fingers of the attendant. . . . The *materies morbi* most capable of being inoculated . . . seems to be the inflammatory products . . . of females who are suffering under puerperal fever or who have died of it . . . and lastly other inflammatory effusions into tissues which are the seat of an asthenic erysipelatous or gangrenous type of inflammation. . . . .

With reference to Vienna, he said: "The mortality altered and diminished immensely and immediately from the time (May, 1847) that the assistant physician, Dr. Semmelweis, prevented students from touching parts at the autopsies, and directed all of them to wash their hands in a solution of chlorine before and after every vaginal examination. . . ."

Simpson goes on to speak of mothers sacrificed to medical prejudice in the form of a total disbelief on the part of our continental brethren in the contagious communicability of puerperal fever.

Simpson's article appeared just after Semmelweis had returned to Buda-Pesth, and the same month as Arneth left Vienna on the journey which ultimately brought him to Edinburgh.

In May, 1851, Arneth addressed the Medico-Chirurgical Society of Edinburgh on the "Evidence of Puerperal Fever depending upon the Contagious Inoculation of Morbid Matter," and the address was published as an article in the *Monthly Journal of Med. Science* in June.

This Edinburgh episode amounts to the most important event in the history of the spread of the Semmelweis doctrine. Owing to the position which

Arneth had held in the Vienna Lying-in Hospital and his associations with Semmelweis the recent discovery and the teaching which depended upon it might as well have been proclaimed fully and completely at first hand to the English-speaking medical world. It was only ten months since Semmelweis finished his series of addresses to the Vienna Medical Society.

Dr. Arneth gave a summary historic account of the mortality of the Vienna Lying-in Hospital from the foundation, and its variations concomitant with the amount of dissection and postmortem work done by the students of midwifery and assistants in the First Obstetric Clinic.

He traced the experiences which brought Semmelweis to the conclusion that puerperal fever was caused by inoculation of the patients with cadaveric poison, and then the extension of observations which led to the complete form of the etiology. " The opinion of Dr. Semmelweis on this important matter (the process of absorption) is as follows: Any fluid matter in a state of putrefaction—communicated by linen, by a catheter, by a sponge, by small particles of the placenta, even by the ambient atmosphere impregnated by the foul substances —may produce puerperal fever."

Among facts which appeared to Arneth to speak strongly in favour of the opinions of Semmelweis were the experience of Michaelis at Kiel, and the reports from the whole of those lying-in hospitals of the Austrian Empire where midwives only were trained showing that no epidemic of puerperal fever ever made its appearance. One remarkable exception to the rule was that of the lying-in hospital of Pavia, where the pupil-midwives used to dissect in one of the rooms of the lying-in hospital the bodies of children who died in the hospital.

". . . It is my firm conviction that any individual who may not choose to submit to what we have stated to be necessary after the contact with matter in putrefaction may artificially produce the puerperal fever in many persons whom he may successively examine, and who in all probability would never have become ill but for this inhuman carelessness. . . . I know of no case of the puerperal fever having been communicated through the clothes of a medical man to a patient. As far as I know there is no such case on record, neither in hospital nor in private practice in Vienna. . . ."

Arneth expressed his disbelief in erysipelas as an etiological factor in puerperal fever, and he said they did not find the children of the puerperal women seized with erysipelas. "Scarcely one of our pupils, and none of the nurses, were taken with erysipelas during my stay in the Vienna hospital."

Speaking of the English belief in the importance of contact with erysipelas in bringing on puerperal fever, Arneth called attention to the fact that nearly all the illustrations, given in English periodicals and standard works, are cases of gangrenous erysipelas causing puerperal fever by the production of decomposed animal matter, which was brought into direct contact with the sexual organs of the puerpera.

Here then we have the complete Semmelweis doctrine of infection by direct contact expounded by one who knew it in every detail and published in an important British medical journal. We have also the rôle of erysipelas as an etiological factor finally appraised, and yet we shall find the tenacity of error exemplified to a very remarkable degree in the Obstetrical Society of London a quarter of a century later. Among the speakers on that occasion there were several who proved that they thought clearly and held scientific consistent modern opinions, and these were almost, or altogether, men who had been trained in the Edinburgh school of medicine.

Chronologically, Routh was the first to call the attention of the medical profession in the United Kingdom to the Semmelweis discovery and doctrine, but he was young and inexperienced, and he did not grasp the full import of the discovery as Arneth could. Hence it resulted that the true pathology of puerperal fever was first taught with clearness and conviction in Edinburgh and Buda-Pesth, and these schools of medicine had a start of more than two decades over the rest of Europe.

In the end of 1851 Chiari published a paper in the *Vienna Medical Journal* on "Pyæmia in the Puerperium without Uterine Lesion." The contribution is interesting as containing for the first time a description of puerperal fever marked by repeated rigors, with the results of postmortem examination.

It was declared by the opponents of Semmelweis that Chiari had given up the infective theory, but this was not the case; the Semmelweis doctrine had no more sturdy and consistent supporter from first to last than Chiari.

v. Waldheim mentions as a curiosity among opinions that of Dr. Rosshirt, professor of midwifery at Erlangen, whose textbook on midwifery appeared in this same year 1851. Rosshirt maintains that during pregnancy the condition of the uterus is that of being more or less in a state of inflammation. "The frequently occurring inflammation of the peritoneal covering of the uterus is explained by the rapid stretching and distension of the peritoneum."

The only curiosity about this opinion is the extraordinary tenacity of the hold that it maintained on the obstetric mind; irrationality would be nothing unusual. It was an opinion quite generally prevalent, as we shall see, in England in the later decades of the eighteenth century. Erlangen was evidently in Rosshirt's time far removed from the high-ways of scientific progress: it has seen better times since then. Schoeder was to arrive after Rosshirt.

In 1851 Kiwisch returned to the subject of puerperal fever in a new edition of his work on the diseases of the female sexual organs. He only proves that he has made no progress since the attack on Semmelweis and Skoda two years previously, which Hegar deplored as a remarkable example of "human error." It is the prominent position of Kiwish as a German teacher of midwifery, not the intrinsic weight of his opinions, which calls for a few remarks. According to this last pronouncement puerperal fever is a feverish disease *peculiar to lying-in women*. It is of miasmatic origin, and the first phase is a diseased condition of the blood; from this arise the local, chiefly inflammatory symptoms. Its occurrence as an epidemic simultaneously over whole countries, and even over the greater part of the Continent of Europe has been demonstrated beyond question. . . . There is no local condition which can be considered an important causal factor in the production of puerperal fever, but it appears to depend upon an entire change in the individual resulting from the puerperal state, in which there is a general shock to the whole body, and especially to the nervous system. . . . We encounter once more a dissertation on English opinion regarding contagiosity, and the intimate relation of puerperal fever with erysipelas which the writer appears to sympathise with.

Referring to Semmelweis and Skoda he says that they have now spoken more definitely than formerly on the origin of puerperal fever in cadaveric poison. But the fact was that neither Semmelweis nor Skoda had contributed either written article or formal spoken word to the subject since Kiwisch's publication of two years before. Kiwisch appears only to have read more carefully what Semmelweis and Skoda had formerly said with a view to the preparation of his new edition.

Kiwisch declares that he still continues to make post-mortem examinations, and go direct without special precautions to perform obstetric operations, and "I could not in any single case find room for the suspicion that I had thereby caused an outbreak of puerperal fever. . . ." Still in those institutions where there is a possibility of infecting a lying-in woman by decomposed animal material (cadaveric poison, wound secretions, decomposed puerperal effluvia) their influence must be avoided as far as possible, and it is necessary for this purpose to employ the measures in use in England and introduced by Dr. Semmelweis, chlorine disinfection and fumigation.

Thus we see that Kiwisch had actually adopted the Semmelweis prophylaxis, though he still argued against it on theoretical grounds; and though he must have read the Hebra articles and the report of the addresses delivered by Semmelweis at the Vienna Medical Society the year before, he joins in perpetuating the error and misrepresentation that Semmelweis attributed the infection of lying-in women only to cadaveric poison.

If we trace the fortunes of the Semmelweis Doctrine for a few years more we find it is a story of persistent obstruction by misrepresentation with hardly a cheering incident.

Professor Busch of Berlin declared in 1852 that all measures to prevent epidemics of puerperal fever had failed, and that the only effective means of stopping an epidemic was to close the lying-in institution.

In the following year Scanzoni published another volume of his manual of midwifery, and took advantage of the occasion to repeat his old errors with regard to the definition and import of puerperal fever. He excluded in fact almost everything which we now regard as puerperal fever, and included hyperinosis, pyæmic crasis, and blood-dissolution, which we have ceased to regard as realities.

It was here that Scanzoni boasted that he had the distinction of being the first to oppose the Semmelweis Doctrine of conveyed infection. With regard to etiology by far the most important factor is the *genius epidemicus*. This fact is established beyond dispute (*unstreitig*).

Among the publications of 1854 was a "Compendium of Practical Midwifery," by Lumpe of Vienna, whose acquaintance we have already had the pleasure of making. Lumpe's book is worth mentioning as indicating remarkable retrogression under the influences of his environment of the previous few years. Lumpe does not mention Semmelweis from first to last. He says nothing about disinfection, not even about ordinary cleanliness. He had evidently ceased to "wait and wash."

Where the Semmelweis principles were known and put in practice without faith and without zeal and exactitude the results were in some institutions highly unsatisfactory and were quoted as evidence against the *Ætiologie*. For example, Retzius of Stockholm lost 3·3 per cent., and Faye of Christiania 15 per cent. about this time. Among those who seized upon these unfortunate incidents as evidence against Semmelweis was naturally Carl Braun, his successor as assistant in the First Clinic. Semmelweis[1] remarks on the circumstance in the *Stimme*. In very large institutions there are many hundreds of patients who are never subjected to examination for purposes of instruction. "But in an institution where only 150 or 200 births occur annually every case is made use of for purposes of instruction, and in spite of the most excellent arrangements if only one indolent individual is admitted among the students an enormous mortality may result."

Among the witnesses quoted by Carl Braun against the Semmelweis Doctrine in 1855 is Mende of Vienna. "Mende[2] doubts the correctness of the theory of cadaveric infection, and believes that the cause of the frequency with which puerperal fever occurs in Vienna depends upon the difficulties of ventilation, the crowding of many lying-in women in a small building connected with the General Hospital, and the consequent facility for the production of miasma." "But," says Semmelweis, "Carl Braun knows as well as I do that this state of matters has remained unchanged while the mortality has been reduced, and that the cause of the reduction has been his own fight against cadaveric infection."

The hostility of Carl Braun was largely personal. After he became assistant to Klein in succession to

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[1] Aetiologie, p. 426. [2] Aetiologie, p. 526.

Semmelweis he used to refer to the Doctrine of his predecessor as all "humbug." But when it was pointed out to him that from the very month (March, 1849) in which he himself commenced work at the Lying-in Hospital the mortality began to rise, and went on increasing during the year and a half before Semmelweis left Vienna, Braun became a personal antagonist of Semmelweis.

The source of the sudden zeal for ventilation, cleanliness and isolation of sick puerperæ, which now arose in Vienna and had ultimately its chief apostle in Carl Braun, can easily be traced to Arneth, the friend and former colleague of Semmelweis. It all came from the United Kingdom.

Arneth returned from his travels in 1852, and his book on "Obstetrics and Gynæcology in France, Great Britain and Ireland" was published in Vienna in 1853. In the preface he said that after a term of three years as assistant he had undertaken his journey in the West, and found so much of interest that he believed he would be doing a service to others in describing his experiences.

In comparing the Lying-in Hospital of Dublin with that of Vienna, Arneth calls attention, among other points, to the ventilation and the odour perceived in the wards. "The ventilation is effected much more efficiently in Dublin than with us; and in Dublin there was no trace of odour from the privy, whereas with us where for an entire division there is only one small room set apart for this purpose, we are almost always troubled with the privy smell" (*Geruch vom Aborte*).

Arneth also repeatedly refers to the cleanliness and airiness of the lying-in hospitals of London as one of their most remarkable features, *e.g.*, "in so spiegelklaren Häusern wie das City Lying-in Hospital."

His book would be widely read and much discussed in Vienna, and in this no doubt we have the source of the new-born zeal for ventilation and imitation in other respects of English midwifery practice. A powerful attraction to the new methods was also without doubt the relief thereby obtained from conceding any merit to Semmelweis for his *Lehre*.

The first attack upon Semmelweis emanating from Vienna was contained in a work entitled "Clinical Obstetrics and Gynæcology," by Chiari, Braun and Späth, published in 1855. The theoretical portion was the work of Carl Braun. Of thirty causes of puerperal fever discussed the twenty-eighth is "cadaveric infection." It could serve no good modern purpose to criticise in detail Braun's largely antiquated theories on the alleged causes, such, for example, as the first, "pregnancy itself!" It must suffice to summarize what Semmelweis had to say on the subject in the *Ætiologie* under Opinions. For Braun puerperal fever is still a "zymotic disease of acute character, which can be produced in individuals with a marked predisposition, by means of general injurious influences such as emotional disturbances . . . . but as a rule by special influences, miasma, contagion, decomposed animal matter . . . . by which the peculiar foreign influence works as a ferment, and by contact sets the blood-mass in fermentation."

Among statements made by Carl Braun in this first attack, upon which Semmelweis makes little comment, are the following:

"We cannot therefore support to the full extent the thesis brought forward in support of cadaveric infection on the ground of observations made in the Vienna Lying-in Hospital. We cannot lay the blame on manipulations of the cadaver as an important cause of puerperal fever epidemics; we would, however, consider it an act of the greatest presumption to permit or to practise examinations or perform operations on the puerperæ with hands which retained a cadaveric odour after even the most diligent washing. . . . The position of lying-in hospitals shows in the state of health of the patients the mightiest influence. Lying-in hospitals should be separate entirely from general hospitals." Clearly from Arneth, who was much impressed with the fact that

English lyingin hospitals are 'self-contained' institutions and frequently referred to the circumstance . . ." The vicinity of localities which are full of animal material, such as dead-houses, great outflows of sewage, unclean privies, defectively drained, putting the placenta in the privy: all these things assist in the spread of epidemics. . . . The defective construction of lying-in hospitals with insufficient ventilation. . . . The exhalations from foul excrement . . . the puerperal odour (Puerperal-geruch), the failure to isolate the sick from the normal . . . the continuous use of all the wards of a lying-in hospital . . . uninterrupted teaching of midwifery in crowded lying-in hospitals . . . the admission of patients suffering from zymotic diseases into the labour-rooms. . . ." All news in Vienna: all brought from England by Arneth.

Semmelweis devotes the last article of the *Opinions* to Carl Braun's publications of 1855 and 1857 (Lehrbuch der Geburtshilfe), and he appears to linger over the points of his attack with a consciousness of power to annihilate his arrogant rival.

"Carl Braun, my successor as assistant and now professor of midwifery at the First Obstetric Clinic of Vienna . . . . where I discovered the for ever true etiology of puerperal fever, is an opponent of the true etiology of puerperal fever, and owing to the influence he might exercise I am compelled to go more fundamentally into the subject than with some earlier opponents, but Carl Braun himself makes the task easy.

"Carl Braun's opposition to the true etiology of puerperal fever arises not from a conviction that my etiology is not true: his opposition is based partly on his own ignorance and partly on illwill towards myself."

Long quotations are given from Carl Braun's writings to prove that whatever he may say openly about Semmelweis and his doctrine he has practically adopted the prophylaxis entire in his hospital work . . . "Yet he has, contrary to his convictions, not only written against my 'Lehre,' but he is in the habit of lecturing to his students against it, and he has consequently produced a dangerous sort of levity among his students in regard to prophylaxis. What is to be expected from men so badly taught when they assume the responsibilities of independent practice?"

According to Carl Braun there are thirty causes of puerperal fever, of which the 28th in his list is cadaveric infection. In commenting on this etiological factor he says: "As the chief, and in fact as almost the only cause of puerperal fever epidemics, Semmelweis endeavoured in 1847 to establish the theory of cadaveric infection according to which the hands after manipulation of the cadaver become the carriers of infection. . . .

". . . Semmelweis found in Skoda a champion of this doctrine."

Commenting on this disingenuous passage Semmelweis says: "The reader sees how badly Carl Braun has used the opportunity offered him of learning something since he knows only one source of decomposed animal matter, namely the cadaver, and only one carrier of the decomposed matter, the examining finger. . . . The reader knows that there are three sources of decomposed matter, and infection occurs more from one source at one time, more from another source at another time. In Vienna it was undoubtedly the cadaver which was the commonest source of infection. . . . In the hospital of St. Roche at Buda-Pesth the source of infection was the surgical department; and Chiari relates how at Prague two puerperal fever epidemics were produced owing to putrid discharges from the genitals of patients during labour . . . and in the University Obstetric Clinic at Buda-Pesth it was foul bedding and bed linen which supplied the decomposed animal matter.

"The carrier of the decomposed matter is not only the examining finger, but any object which is rendered unclean by the decomposed matter and then comes into contact with the genitals of the individual."

Then follows here once more a clear concise statement of the doctrine and the prophylaxis. (*Ætiol.*, p. 489.)

Carl Braun said, in his book, "During the winter of 1849 a severe epidemic of puerperal fever prevailed in spite of the prescribed chlorine disinfection, and ceased in the beginning of the milder season in April without ascertainable cause. In the summer time only 29 deaths occurred among 1,818 patients, although clinical teaching was uninterrupted and the courses of operative midwifery on the cadaver were conducted by the assistant as usual. In the winter semester puerperal fever broke out again, as it usually does in the autumn, and 77 women died out of 1,888 delivered.

These occurrences must completely shatter the belief in the protective efficacy of chloride of lime. . . ." In spite of the most conscientious and general disinfection of the examining hands the mortality of the epidemic rose from January to March to 5 per cent.

Next summer it was found that the cadaveric odour was still retained by the hands of the students in spite of the prescribed disinfection, so disinfection was given up, and the students were forbidden from examining parturient or puerperal women on the same day as they had occasion to handle the cadaver. Still, in spite of the greatest care the puerperal fever mortality continued to rise, until in March, 1851, it carried off over 7 per cent. of the patients of the First Clinic.

In the School for Midwives where cadaveric infection could not readily be produced, and where the greatest watchfulness was exercised (auf das strengste überwacht) they had the sad experience in January to March of losing 10 to 12 per cent. of the patients from puerperal fever.. "These facts must completely annihilate the hypothesis of cadaveric infection which rested for the most part upon the past and led to some very bold conclusions, and they warn us that we must give consideration to other etiological factors."

In reply to all this Semmelweis, as is his custom, goes into great detail concerning the events in the work of the Vienna Lying-in Hospital for many years, and concludes that Carl Braun has not disproved his statements and contentions, but by the simple expedient of selecting dates he has been guilty of misrepresentation and prevarication. If Carl Braun does not believe in cadaveric infection why did he lay down a rule that students must not examine patients on the same day on which they had handled the cadaver? To deny something and yet to act as if it were true is to perpetuate a falsehood. That the cadaver causes infection and that chloride of lime disinfects Carl Braun himself has proved.

Then with regard to the etiology, Carl Braun observed epidemics of puerperal fever break out without any discoverable cause, and he saw them disappear without any discoverable cause; but Carl Braun mentions in his book thirty causes of puerperal fever! Why does he not follow the lead of Scanzoni? When he saw women dying of puerperal fever without being able to discover a cause he fell back for the etiology upon accident (Zufall).

The facts with regard to the high mortality in the clinics at Vienna when Semmelweis was assistant do not shake his faith in disinfection, they only suggest to him that if the opponents of disinfection have worse results than before the origination of the prophylaxis the opponents do not carry out the process of disinfection so exactly and conscientiously as the originator did.

We have next a long discussion concerning the influence of the seasons of the year as an etiological factor, and Carl Braun's rather exaggerated statements with regard to the occurrence of puerperal fever epidemics not only in lying-in hospitals but over different countries, in cities, and in open country districts, including mountainous regions, while the frightful malady spares no class of society. The reply is that these statements if they were true in no way contradict the theory of conveyed infection. Carl Braun trains from 150 to 200 students every year, and how they are instructed in prophylaxis is shown by the shocking mortality in the First Clinic. Dr. Späth sends out from 260 to 300 trained midwives every year, and how well they are taught with regard to prophylaxis is shewn by the 12 per cent. mortality in 1852. "Thus it is that infectors are trained for different towns, villages and country districts, and for various classes of Society." Besides it is difficult or impossible to obtain statistics of private practice on account of the universal adoption of euphemisms for fatal puerperal processes such as nervous fever, typhus, etc.; and in Austria it is forbidden by law to certify such diseases as puerperal fever and carcinoma in women as the immediate cause of death.

An example of controversial method on both sides occurs in the discussion of the mortality in certain foreign lying-in hospitals. Carl Braun has said that the favourable results obtained in English lying-in hospitals depend upon the fact that only married women are admitted to such hospitals in England, whereas in France and Germany only unmarried women are admitted to lying-in hospitals. "The reasons for the better results in England," says Semmelweis, "we have fully explained, but we never imagined that our prophylaxis was defective because we did not prescribe marriage as a protection against puerperal fever."

We next find quoted a long summary of Carl Braun's explanation of the worse results in his department as compared with the School for Midwives, in overcrowding, difficulties of ventilation, admission of a worse class of cases; anything rather than the admission that the hypothesis of cadaveric infection was worthy of serious consideration.

Semmelweis seizes every point and answers in great detail, with dates and statistics, and many pages are devoted to matter which was of little except personal interest then, and can be of no value from any point of view to the modern English-speaking reader. They only illustrate once more the earnestness, patience and devotion of the man as the apostle of his doctrine. The repeated re-statement and exposition of the complete import of the Doctrine should have made any misunderstanding impossible; yet we shall find more than twenty years after the publication of the *Ætiologie* German writers of eminence describing the doctrine of Semmelweis as one-sided.

Braun next falls back on " authorities " and the " literature" for support. It is over five years since the Semmelweis doctrine was first published to the world, let us hear what the men who know best about lying-in hospitals have to say about it. Among the names mentioned are some men of mark such as Scanzoni and Kiwisch, and two representatives of obstetrics in Scandinavia. The rest are mere nobodies. Carl Braun finds nowhere any confirmation of the alleged reliability of the theory of infection in its practical applications, but we meet everywhere with expressions of the most decided opinions and reports of experience which deprive this hypothesis of its strongest support.

Among the list of authorities mentionel by Carl Braun with characteristic recklessness and disregard for accuracy occurs the name of Chiari, one of the earliest and most consistent supporters of the Semmelweis doctrine. It was the episode of death from the removal of a fibroid polypus of the uterus when Chiari was assistant to Klein that led both Chiari and Semmelweis to reflect on the phenomena and symptoms, to reject the ordinary explanation, and apply chlorine antisepsis to gynæcological surgery.

" It was an extremely painful surprise to me to find Chiari included among my opponents for I had never heard an expression of opposition from him," and although Semmelweis cannot believe that Chiari ever expressed an opinion of the etiology contrary to his own, he proceeds to quote from an address on " Pyæmia in Puerperio" delivered by Chiari in 1851. But it was all a deplorable mistake: Chiari had never wavered in his support of the Semmelweis Doctrine, and the heartache caused by the episode was only the consequence of Carl Braun's peculiar method of controversy. Unfortunately before the misunderstanding could be cleared up Chiari was in his grave, carried off by cholera in 1855.

Among authorities quoted by Braun is Faye of Christiania, who in spite of all advantages of hospital construction, had a mortality of 15 per cent. from child-bed fever. Semmelweis has little difficulty in turning the facts to the support of his doctrine.

Semmelweis next conscientiously examines and partly discusses the thirty causes of puerperal fever enumerated by Carl Braun in his book. He begins by saying in two sentences what might have stood for the whole dreary dissertation : "Braun's etiology is partly error and partly truth : Braun's etiology is error when he teaches something different from my teaching, it is the truth when he teaches what I teach."

"In Vienna I established the truth of my doctrine by its practical application. If I had not been able to prove it true by its practical application during my term of service, that would not have made it erroneous. What is true in Vienna is true over the whole world, and if the truth has not been made manifest elsewhere that does not turn the truth into falsehood, and only proves the incompetence of those who have failed."

"Did Auenbrugger or his contemporaries prove themselves incompetent because Auenbrugger did not live to see percussion brought into general use ? "

In Carl Braun's summary of the etiological factors of puerperal fever we find references to practices peculiar to the Continental lying-in hospitals of those days which make the modern reader shudder. Professor Simpson's *Schmähungen* in reply to Arneth's letter about the Semmelweis discovery in 1848 were only too well grounded.

Semmelweis concludes his chapter on Carl Braun by giving, and remarking on, the revised definition of puerperal fever contained in Braun's book. "Only the definition of child-bed fever shall we give because it is once more a demonstration of how far astray an undigested compilation may lead. We may reflect that the passage is interesting as throwing light on the principles taught then by the most prominent professor of midwifery in Europe and director of the largest lying-in hospital in the world. According to Carl Braun puerperal fever is a zymotic disease of an acute character which may be produced owing to strong predisposition in an individual, and also by general injurious influences such as emotional shocks (Gemüthserschütterungen) chilling and such like; but as a rule it is produced by special influences through miasma, contagion, decomposed animal matter, whereby the peculiar foreign material acts as a ferment, and by means of contact sets the blood-mass into a state of fermentation."

"The reader will see with astonishment that Carl Braun who so brilliantly combated the hypothesis of cadaveric infection, who so triumphantly vindicated . . the unlimited power of epidemic influences: that the same Carl Braun now assigns a place in the conception of puerperal fever even to decomposed animal matter not to epidemic influences. Oh, logic."

VIRCHOW. Considering the vast services which Virchow rendered to medical science it would be unbecoming to take the position of a partisan and unduly condemn him for the way in which he acted at the first towards Semmelweis; but it is nevertheless a deplorable incident in the history of midwifery. Virchow was a great pathologist, like Rokitansky: he had no real qualification to speak as an authority on Obstetrics. Perhaps the fact that he was the son-in-law of a professor of midwifery, who founded the Obstetrical Society, gave him a confidence which the amount of his exact knowledge did not justify.

It is a pity that Virchow did not take the trouble to learn something of the history and personality of Semmelweis; there must have been some affinity and similarity in temperament between the two men. At the time when Semmelweis was wearing the uniform of the revolutionary Academic Legion in Vienna, Virchow as a revolutionist was fleeing from Berlin for life and freedom in exile beyond the boundaries of Prussia.

Virchow ultimately came round to appreciate the Semmelweis doctrine, but his influence in the meantime had done much to retard the practical adoption of the principles of Semmelweis in Germany.

Virchow's adoption of the doctrine of Semmelweis was declared in 1864, but his support came too late to comfort the unhappy author. It was the opinions published in 1856 which roused the resentment of the author of the *Lehre*. Semmelweis (*Ætiologie,* p. 468) says:

"Rudolf Virchow says, in his collected contributions to scientific medicine, "Natural science research knows no more frightful image than the person who speculates (*als den Kerl der speculirt*). Boër had formulated the same truth in this way, when speaking of Hippocrates; "If in every century instead of so many system-builders one observant physician such as he had arisen, how much more would have been gained for mankind."

Boër, the author of the seven books on Natural Labour, had a right so to speak. But Virchow, who on account of his numerous speculations, is himself a frightful image for Natural Science research; Virchow, who is such a bad observer that as pathological anatomist he could not even in the year 1858 recognise the traces of a resorption fever in the cadaver of a victim to childbed fever; Virchow has no right to speak unless according to his humour in a moment of jovial straightforwardness he sought to characterise himself.

"The expression of Virchow about the fellow who speculates" stands amidst a long series of contributions on puerperal diseases which Virchow seeks to supply but is unable to supply; he speaks in his introduction about menstruation, conception and pregnancy as of things which stand in some causal relationship with puerperal fever.

The anatomist, the surgeon, the subject of a surgical operation, the newly-born suckling male or female, who dies of puerperal fever or pyæmia in my sense of the term, has never menstruated, has never conceived, nor ever become pregnant, and yet he dies of the same disease of which the lying-in woman dies, . . . and my doctrine is not based on the art of putting a stop to menstruation, conception, and pregnancy.

" Pregnancy supplies for puerperal fever nothing except an absorbing surface. . . . . In the lying-in woman the absorbing surface brings no puerperal fever when this surface is not rendered unclean by a decomposed material, and the unessential nature of the internal absorbing surface for the production of puerperal fever is proved by this fact that the smallest injury in any part of the male or female body may be sufficient to produce the disease."

" Virchow says: For the occurrence of puerperal fever epidemics two conditions of interest are essential: the state of the weather, and the occurrence of certain diseases simultaneously. In regard to the first, it appears that the largest number of epidemics occur in the winter months. To the simultaneous diseases belong the acute exanthemata, the extensive spread of erysipelatous, croupous, putrid and purulent inflammations.

"It is quite correct that the largest number of epidemics occur in the winter months, not because of the weather conditions of the winter, but because the winter is the time when occupation with decomposed animal matter is chiefly carried on." [Reference is made here to Table XIX., p. 120, as a demonstration that the state of the weather does not exercise any influence in the production of puerperal fever.]

" It is also quite correct that puerperal fever occurs simultaneously with acute exanthemata, with widespread erysipelatous, croupous, putrid, and purulent inflammations, and the cause of this simultaneous occurrence is that such patients are treated by the same medical practitioners and midwives as treat medically and nurse pregnant, parturient and puerperal women."

Instruction in obstetrics like that of Virchow, who uttered his opinions on puerperal fever epidemics in an address at Berlin in 1858 without a word of protest from any quarter, such teaching is so absolutely bad that it ought to be suppressed by law.

But why should the teaching of midwifery in Berlin not be bad when Professor Schmidt believes in a nosocomial atmosphere?"

Professor Credé is an epidemicist, and in support of his theory in the Winter Semester of 1854-55 he sent out of 336 lying-in women 58 to other stations to die there. Credé's transference to Leipzig did not change his opinions in any way: there he lost over 3 per cent. of patients from puerperal fever for three years running.

"Professor E. Martin, of Berlin, shows, from opinions stated in the Berlin Obstetrical Society in 1858 as to the cause of puerperal peritonitis, that the "puerperal sun" which rose in Vienna in 1847 has not as yet lighted up his spirit."

It will be observed that the reference to E. Martin is conceived in the most forbearing terms ever used by Semmelweis, for, as we shall see, E. Martin was the most reasonable of opponents, and was among the first in North Germany to appreciate the importance of the Semmelweis *Lehre*.

Virchow had not attacked the Semmelweis doctrine in any way: he had treated it with a disdainful silence while diligently spreading opinions about puerperal fever which Semmelweis believed to be erroneous and harmful. So great was the position of Virchow as an oracle on all subjects in the opinion of his countrymen, that the professors were simply shocked at the temerity of Semmelweis in criticising the pronouncements of their divinely gifted man. Semmelweis tackles the great man in a special article, "Die puerperalen Thrombosen."

Virchow had stated the belief that the less sufficient the contraction of the uterus, and of the vessels in the neighbourhood of the uterus, the greater is the danger of the formation of physiological thrombosis, and of the transformation of the thrombus into puerperal fever, and *vice versâ*. Among the agencies which produce firm contraction of the uterus, according to Virchow, is "in all probability" a special nervous influence. This is probably the reason why women who are confined secretly (heimlich), and who are consequently subject to such nervous excitement, are so seldom the victims of dangerous accidents, whilst we often see such untoward results in the case of weakly women in spite of the best nursing, and still more in overcrowded lying-in hospitals under miasmatic influences. So Virchow believes that nervous excitement and lactation prevent puerperal fever.

Kiwisch says: "With regard to milk secretion, it was my experience that non-suckling women, during an epidemic, were more seldom attacked than the women who suckled. . . . In the portion of the Prague lying-in institution reserved for paying patients, in which no woman gives the breast to her infant, the proportion of sick puerperæ was much smaller than in the division for suckling women."

Scanzoni finds in nervous excitement just the cause of a higher mortality among the patients of an institution devoted to the education of students of medicine, as compared with a school for midwives. And Professor Braun is of the same opinion as Scanzoni.

But women confined in concealment, or in the paying wards at Prague, are not made material for clinical teaching purposes: therefore they are not infected; in schools for medical men the patients are more frequently infected than in schools for midwives, hence the relatively unfavourable condition in the former.

"It is ridiculous for a set of men such as these to pass judgment upon conditions which they do not understand. Veit's declaration that "The mortality of the Vienna Lying-in Hospital affords a shocking example, I repudiate with all the indignation of which I am capable. The mortality of the Vienna Hospital was not more shocking than that of other institutions where similar conditions prevailed, and for a misfortune springing from general ignorance no one can be made responsible. But the shocking mortality of the Vienna Hospital brought about the discovery of the means by which the mortality from puerperal fever can be restricted to less than one in a hundred; whereas the shocking mortality at other places has had no other result than the filling of the dead-houses. What right has Veit to speak of the shocking mortality of the Vienna Lying-in Hospital, who still in the year 1855 opposes the methods by which the shocking mortality is prevented? This is the same Veit who attributes the shocking mortality to atmospheric influences beyond our control and thereby condemns lying-in women to this shocking mortality for ever.

"With what right does Virchow lend the authority of his name to such opinions, the same Virchow who certainly has not attacked my doctrine because in his calm *Ueberhebung* (arrogance) he remains entirely ignorant of it, and is therefore stuck in such ignorance of the origin, the import and the prevention of childbed fever that in the year 1858, in the Berlin Obstetrical Society, he gave an address on the puerperal diseases at the *Charité,* and in it he admitted that the epidemic in the month of November caused 20 deaths? He did not appear to reflect what a shocking, and at the same time what a criminal, mortality this was, occurring as it did eleven years later than the time when in Vienna they had learned the means by which the mortality might be brought down to under 1 per cent. . . . This fact is in some degree an indication of the shocking state of midwifery teaching in Berlin. . . .

"To say nothing of my students, of medical practitioners and surgeons, there are at the present time 823 of my pupil-midwives carrying on midwifery practice in Hungary, who know better than Virchow why the majority of epidemics of puerperal fever occur in winter, who know better than Virchow what to do in order to prevent puerperal fever when patients suffering from erysipelatous, croupous, putrid and purulent inflammation are committed to their care; who, more enlightened than the members of the Berlin Obstetrical Society, would laugh in derision at Virchow if he gave them a lecture on epidemic puerperal fever."

HAMERNIK.

Just as Bamberger, professor of medicine at Würzburg, was influenced to take a feeble part in the discussion against Semmelweis, so Professor Hamernik of Prague also intervened. Hamernik had been selected by the authorities as a member of a Commission appointed at Scanzoni's suggestion to inquire into the whole question of puerperal fever. The commission was appointed in 1849, and in 1860 Semmelweis could write: This commission has not yet communicated to the world the solution of the riddle of puerperal fever."

Hamernik is only worth quoting because of one passage which forcibly illustrates one of the unscrupulous methods of opposing Semmelweis. "We would now only call attention to the fact that nowhere is the fear of puerperal fever greater than in England and Russia, which is the result of the wide-spread and murderous epidemics which terrify the people and the medical profession in these countries. Yet in England and Russia for a long time past no autopsies have been made, and in England in particular the body of a puerpera is never opened."

Semmelweis replies: "That it is a mistake to believe that England is specially afflicted with puerperal fever the public reports of English lying-in hospitals, which have been quoted, demonstrate. We have also explained why the mortality from puerperal fever is so small in England. If, in spite of this low mortality, English medical men have a greater fear of puerperal fever, that is only a proof that the English medical practitioners are more conscientious than they are in other countries."

This quotation from Hamernik is a fair example of his matter: circumscribed knowledge, reckless misstatement of facts, animus everywhere; yet there is always the unfailing patience in Semmelweis's reply, enlightened occasionally with a touch of irony.

Liebig. In support of his contention against Seyfert of Prague, Semmelweis appealed to the authority of Freiherr Gustav Liebig, the great chemist. Liebig says it is a fact that the cadaver in the dissecting room often reaches such a degree of decomposition that poison may be communicated to the blood of the living. The slightest wound from a dissecting scalpel has been known to produce a dangerous condition. . . .

After quoting Skoda's address to the Imperial Academy at Vienna in 1849, Liebig goes on to say: "From this address it incidentally appears how little has been the recognition outside of the Academy of this great, practically important, discovery. Certainly numerous causes of puerperal fever have been specified . . . but that which has been discovered by Dr. Semmelweis, with all the acumen of an unbiassed investigator, . . . cannot be doubted by any unprejudiced man. The only cause of puerperal fever is decomposed material: there are three sources of decomposed matter and one of these is the cadaver."

In reply to a question as to the reason for omitting this passage relating to the Semmelweis discovery from the next edition of his "Letters," Liebig replied (March 21, 1859): "It was not because I do not as formerly appreciate the value of your experience, but because your discovery is now so widely known that there seemed to be no reason for retaining the passage. . . . Chloride of lime (*Chlorkalk*) undoubtedly possesses disinfectant properties."

Anselm Martin. Professor Anselm Martin, Director of the Lying-in Hospital of Munich, wrote in 1857, on the causes of epidemic puerperal fever (*Monatsschrift f. Geburtskunde*). Martin gave an account of an epidemic of puerperal fever which had broken out in the newly-constructed Lying-in Hospital at Munich in December, 1856, and continued till the end of June, 1857. The mortality was high. According to Semmelweis this incident is of special interest, because in seeking for the causes of the sickness, all the old alleged factors—unfavourable locality, overcrowding, want of cleanliness, and so forth—must be excluded. The structure and furnishings were considered perfect, and the administration endeavoured by cleanliness, ventilation, alternation of the use of the wards, and by the strictest regulations, to prevent the occurrence of puerperal fever. Isolation of sick patients was carried out and the sick puerperæ were nursed by an entirely separate staff. The institution was much admired by visitors from Germany and from foreign countries.

After expressing his own satisfaction with the hospital, Martin goes on to speak of the duty laid upon such institutions to investigate the causes of puerperal fever. About this sentiment Semmelweis remarks: "The time for further inquiry into the etiology has gone by: the only etiological factor for all cases without exception is the access of a decomposed animal organic material. Now the time has arrived for the utmost endeavours to make this single etiological factor so innocuous that in the whole world, both inside and outside the lying-in hospitals, the disease may be seldom met with. . . . As a rule it may be said that treatment is not successful: we must therefore trust to prophylaxis."

Martin mentions as causes of puerperal fever matters that do not stand in any causal relationship to that malady, but he admits that some recognise as a cause of puerperal fever, especially in lying-in hospitals, cadaveric particles adhering to the examining hands of physicians and students after making post-mortem examinations, and that even the cadaveric odour remaining after ordinary washing may imply a putrid atmosphere capable of producing puerperal fever. Even the cadaveric smell of clothing, bedding, etc., may cause infection. Still many authorities in the science of obstetrics deny that this can be a cause of puerperal fever, and Martin, while disavowing partisanship on one or other side, proceeds to mention some facts of his experience at the new lying-in institution of Munich.

"After being tolerably free from the disease in the months of January and February, two patients were suddenly attacked on the same day with all the appearance of epidemic puerperal fever. They had both been cases of perfectly normal labour, and there was no apparent cause of the disease. After careful investigation it was discovered that an assistant, without the knowledge of the director of the hospital, had performed a post-mortem examination in the dead-house, 'which was remote from the labour-room,' and after careful washing and disinfection with chlorinated water had gone direct to the examination of these two parturient women. The assistant also admitted that an exactly similar incident occurred in December just before the first appearance of puerperal fever in the new hospital. We then have from Martin an account of an "epidemic," and of the occupation of students, even of those resident in the Lying-in Hospital: they attended the other clinics, even the cases of typhus fever, and they practised in the anatomical department to such an extent that the air of the labour ward had sometimes a smell of the dissecting-room. 'To watch and prevent this state of matters is impossible.' Such practices have prevailed always since 1824, and yet, in spite of the unfortunate condition of the Lying-in Hospital then, epidemic puerperal did not occur.

"On the present occurrence the Lying-in Hospital was closed for a time. 'With the arrival of the better season of the year the epidemic completely ceased.'

"When the clinic began to be frequented by students again some rapidly fatal cases once more occurred, but they ceased at the end of the 'semester' when students ceased to attend the practice of the hospital. Yet the professor actually remarks on these facts: any relation with an infection by the students in these few cases is not admissible; they appear to be sporadic cases such as we often find at the end of an epidemic."

Semmelweis, on the other hand, says: "No commentary on these observations is necessary. . . . The Lying-in Hospital of Munich is a striking proof that there can be no complete safety for the patients even with the most admirable fittings and construction of an institution. What is required is a law in all countries requiring abstention from all practices bringing those engaged in midwifery work into contact with decomposed animal organic matter. Under present circumstances, even when most students are well disposed and careful, the levity of a single individual may bring about the greatest calamity."

MURPHY. In 1857 Dr. Murphy, Professor of Midwifery at University College Hospital, sent an article to the *Dublin Quarterly Journal* which appeared in the August number. The subject was "Puerperal Fever" with a long title, and the paper covered a large amount of space. The relevant matter for our purpose came under Prophylaxis. He discusses ventilation in its bearing on the prevention of puerperal fever, and speaks of the Continental lying-in hospitals as remarkable instances of the neglect of the principles of ventilation. He devotes some space to the praise of chlorine as a disinfectant, and says that Dr. Collins when he became Master of the Rotunda found puerperal fever rife, and he completely expelled it by cleanliness and chlorine fumigation.

Dr. Murphy gives an account of the Semmelweis prophylaxis as practised in the Vienna Lying-in Hospital, and shows an intimate knowledge of its method and results. Chlorine is both deodorizer and disinfectant, and it destroys the effluvia of puerperal fever.

We have here once more the exposition of the Semmelweis doctrine and practice in an influential medical journal, in a paper which would be summarised in the Continental special journals, and yet we shall read in the addresses of Continental professors of midwifery expressions implying ignorance or affectation of ignorance for many years to come.

Murphy's paper appeared at the time when Semmelweis was rousing himself to action in the preparation of the

Ætologie, but it does not appear to have reached him in any form.

When Semmelweis had resolved to prepare for publication an account of his Doctrine, he set to work in feverish haste to collect opinions from the chief teachers of obstetrics in Europe. He had been watching for years the incidence of puerperal fever as recorded in the medical journals at home and abroad, and he knew where to apply for information not yet published.

Levy. In the spring of 1858 he addressed a letter to Professor Levy, of Copenhagen, requesting some account of his experience of the incidence of puerperal fever during the last ten years. Semmelweis knew that Michaelis, of Kiel, who was the first professor of midwifery to accept and put in practice the principles of the *Lehre,* had sent a translation of the letter from Vienna to Levy in 1848. Michaelis had also translated into German and written an introduction to an account of the " Practical Teaching of Midwifery in London and Dublin," published in the *Bibliothek for Laeger,* written by Levy after his return to Copenhagen from London. Semmelweis was so pleased with the introduction of Michaelis that he wrote in the *Ætiologie* (p. 152): "I cannot refrain from giving verbally the introduction of the translator of this report." Michaelis called attention to the efforts made by English obstetricians to banish puerperal fever from their institutions, and the large measure of success which had crowned their work. He also referred to the discovery of Semmelweis, and spoke of the happier times in store for the lying-in hospitals. Levy, who had come in some measure under British influence, was now a sort of contagionist, but not quite emancipated from the old errors of the epidemicists.

On receipt of the first letter from Semmelweis in 1858 Levy replied by referring him to the publication of 1848. He added by way of supplement that the Copenhagen students have no longer anything to do with dissection during their six months' clinical course of midwifery.

"Only in a few exceptional cases, when the *cause of death is not puerperal fever,* do we ourselves make post-mortem examinations, taking care, however, that we do not willingly on the same day explore women in labour. . . . . No precautions are taken except ordinary cleanliness; it is a very unusual thing for the staff to employ chlorine disinfection."

Semmelweis replied that he considered it more important to know what Levy believed to be the truth now after ten years' experience, than what were his doubts ten years ago. He was quite well informed about the prevalence of puerperal fever at the Copenhagen Lying-in Hospital to such an extent as to have threatened the existence of that institution. He knew also that Copenhagen now possessed a new and excellent lying-in hospital; he would like to know why they had built a new hospital, and what had been their experience in it.

Semmelweis devoted many unnecessary pages to Levy's letters: in his usual pathetically patient way he went over the whole ground again, including in his argument even some of the well-known tables of statistics from the Vienna Hospital. It was a waste of time. The impression produced by Levy's letters, quoted in the *Stimme,* is that they were the expressions of a quibbling pedant who had finally made up his mind; he knew much better about everything than Semmelweis could possibly know, and was not open to instruction from him. He knew about the contagious nature of puerperal fever long before he heard of Semmelweis. . . . Neither the observations mentioned, nor the opinions founded upon them, are expressed with the clearness and precision that could be desired in treating of such weighty questions of etiology. There is endless verbosity out of which emerges now and again some evidence of crass ignorance of the first principles of the discovery which he refuses to consider—five questions concerning the exact cause of death in the cadaver from which infection was supposed to be derived, questions about " persons predisposed to infection," and so on. "Puerperal fever has certainly some resemblance to pyæmia, but it assumes many other forms; it was to be regretted that such questions as the sources, the nature, and the effects of cadaveric material had been studied with such *Indifferentismus*!"

Semmelweis replied that in order to answer all Levy's questions it would have been necessary to make experiments upon the patients: "We would rather know less, and preserve the health of our patients more."

This correspondence with Professor Levy, which receives so much attention, is absolutely of no importance or interest except as illustrating once more the outrages upon his feelings to which Semmelweis was subjected by official superior persons who had ceased to learn, and so many of whom appear to have occupied the Chairs of Obstetrics and Gynæcology at that time. We can imagine the enthusiast in the cause of humanity looking across Europe from its educated margin for signs of the spread of his life-saving and eternally true Doctrine, and from an obscure mediocrity, to whom has been mistakenly attributed undue importance, comes the arrogant accusation of *Indifferentismus* and inefficiency in applying methods of investigation!

LITZMANN. Among the professors to whom Semmelweis applied for some account of their experience was Litzmann, the successor of Michaelis at Kiel. It should be remembered that Carl Braun had repeatedly quoted the authority of Litzmann, and erroneously attributed to him the first introduction of chlorine disinfection into midwifery practice in Europe; and we may assume that Litzmann had already formed some preconceptions corresponding to those of Carl Braun, and therefore was not favourable to Semmelweis.

Litzmann's letter is only worth quoting as a monumental piece of self-satisfaction. With regard to puerperal fever he had been more fortunate than his predecessor. "My assistants must not take any direct part in post-mortem examinations, and the students must observe the well-known prophylactic regulations . . . But the chief cause of the better results lies with the care taken to prevent over-crowding. . . . This foresight Michaelis did not exercise." Certainly it must be admitted that in spite of all their foresight they had not been spared the visitation of the smaller epi- and en-demics, and twice they had to resort to temporary closing of the hospital. . . . " Further, I would remark that sometimes the puerperal fever showed itself first in the town or neighbourhood, and then occurred in the institution or the institution was even entirely spared."

This experience of Litmann's that puerperal fever first showed itself in the town before appearing in the hospital is probably the first of the kind recorded in the history of midwifery. We shall hear a good deal more on this subject, especially from the opponents of Semmelweis at Prague and Vienna.

STEINER. Early in 1858 Semmelweis received a letter[1] which must have brought him much-needed comfort. The communication is unique in giving some account of the puerperal infection question from the medical undergraduate standpoint. The writer, Joseph Steiner, had been a student of Semmelweis at Buda-Pesth, and he then went to Gratz to complete his studies. He wrote: " Infection of all sorts occurs at the Gratz Lying-in Hospital. . . . The dissecting-room is the only place where the students can meet and pass the time when waiting for their midwifery cases, and they often devote their attention to dissecting, or studying and manipulating preparations. When they are summoned to the Lying-in Hospital, which is just across the street, they do not make any pretence at disinfection: some of them do not even wash their hands. . . . A diligent student of anatomy is a highly dangerous person for the lying-in women at the Gratz Hospital. . . . The patients might as well be delivered in the dissecting-room. As it is, the students cross the street with hands wet and bloody from dissecting; they dry their hands in the air, and stick them a few times into their pockets and at once proceed to make examinations. . . . It is no longer a riddle to me why after a clinical meeting the Medical Officer (Stadtphysikus) of Gratz exclaimed: 'The lying-in hospitals are really nothing but murder-institutions!' "

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[1] *Aetiol.* p. 410.

Apostrophising Scanzoni in reference to the letter of Joseph Steiner, Semmelweis exclaims: " If Fate had placed this candidate in surgery in your place, Herr Hofrath, I believe that the valleys of Germany would to a less extent re-echo the groans of lying-in women dying of the childbed fever caused by your disciples, male and female, whom you have sent forth into practice from the Lying-in Hospitals of Prague and Würzburg in colossal ignorance of the cause and prevention of puerperal fever."[1]

DISCUSSION ON PUERPERAL FEVER AT THE ACADEMY OF MEDICINE OF PARIS IN 1858.

It was now ten years after the first published announcement of the Semmelweis Discovery, and the subject of puerperal fever was receiving much attention at all the medical schools of Europe, and by all the obstetrical societies.

At the meeting of the Paris Académie de Médecine in February, 1858, M. Guérard mentioned a fatal case of puerperal fever after abortion, and said that he proposed to call the attention of the colleagues to the nature, the mode of propagation, and the treatment of puerperal fever, and requested the help of the members in throwing light upon the nature of this grave malady. This was the origin of the memorable discussion which occupied the Academy for several months, and created a profound impression upon *le monde médicale* of Paris and indeed throughout the whole of France.

The first to address the meeting of the 2nd of March, when the discussion commenced, was Depaul. He began by asking a series of questions which were in all men's mind: Is there a general malady which may be designated puerperal fever? In what does it consist? What is the value of measures of treatment? What means can we employ to prevent it?

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[1] *Aetiologie* p. 413.

These questions have a remarkable resemblance to those formulated by Sir Spencer Wells in opening the discussion of the Obstetrical Society of London in 1875. Depaul referred to the observations of Tarnier, described in the thesis quite recently published, to the evidence advanced by Wieger, of Strassburg, on the work of Semmelweis at Vienna. He also gave an account of some very remarkable observations of his own. As to the contagiosity, there can be no room for doubt. The disease is strongly analogous to purulent or septic infection, but it is neither a pyæmia nor a septicæmia. Treatment is completely powerless to influence the course of the disease: our only resource is prevention. Depaul can only suggest smaller maternity hospitals as a palliative.

The next speaker, Beau, controverted the opinion of Depaul that puerperal fever is a special fever in which the local lesions are secondary: it is rather essentially a local inflammation to which the economy is predisposed by a special constitution of the organism, including augmentation of fibrin. As to treatment Beau believes in large doses of quinine.

Hervez de Chégoin maintained that puerperal fever is nothing but an infection of the blood, as Dubois has also recognised. It does not precede the labour, but follows it, and manifests itself under a putrid or purulent form. Its site is the uterus; one might say with truth, "*morbus totus ab utero procedit.*" From this point of view there is nothing strange or inexplicable about puerperal fever.

Trousseau next gave a long address extending in part over two sittings. He asked whether it was not possible that puerperal fever did not exist as a malady special to the lying-in woman. He recalled episodes in the history of puerperal fever epidemics in Paris and how in 1855–56 the mortality rose to such a height and assumed such a malignant form that Dubois was compelled to close the hospital. He concluded that puerperal fever is a specific disease, but it does not attack lying-in women only. It may attack non-puerperal and even non-pregnant women : it may attack the fœtus, the new-born infant, and even men wounded in any way.

Paul Dubois intervened in the discussion on the 30th of March. This intervention, he said, was imposed upon him more by his professional position rather than by his ability to shed light upon an obscure and difficult problem. . . . The pathological condition which was under discussion presented itself under two principal forms : the bilious and the inflammatory, the latter form including peritonitis, metro-peritonitis, ovaritis. . . . It was not impossible that the diseases with these local affections are produced under epidemic influence. . . . "I admit the primitive alteration of the blood by a cause as yet unknown because this hypothesis appears to me permissible and because after the ruin of the other theories it is the only one which I am able to accept."

The rest of this first address contains a series of pretentious generalities without weight. . . Trousseau's opinion is not new : it is ten years since Simpson of Edinburgh published an important article in which he tried to prove an analogy between a woman recently confined and a person who has just undergone an important operation. . . . After a further series of platitudes Dubois referred to the work of Semmelweis, but he had only heard of cadaveric infection, and the alleged results of disinfection after *des recherches cadavériques*, and he concluded with the dictum : "This opinion is no longer supported in Germany."

Cruveilhier, Physician to the Maternité, was the first to say something valuable and modern in the first month's debate. His opinions amount to the following : 1. Puerperal fever is essentially a wound fever. 2. The condition of the lying-in woman constitutes what might be designated puerperal traumatism. 3. Epidemic puerperal fever is contagious; it should be termed *puerperal typhus.* 4. The essential anatomical characters are peritonitis, lymphangitis, and purulent phlebitis.

Danyau, Assistant Surgeon at the Maternité, a member of the committee that never reported formally on Arneth's address in 1851, said: "For me puerperal fever is a malady of miasmatic origin." As was to be expected of a member of the Maternité staff, Danyau did not think that epidemics were restricted to lying-in hospitals: they often extended over a whole town: the malady might attack women before or during labour. The allegations concerning the transmission of the disease by the medical attendant, as mentioned by Semmelweis of Vienna, are not convincing, and appear to have found few supporters in Germany. . . .

Danyau did not like Tarnier's statistical comparison of the incidence of puerperal fever in the Maternité and in the *arrondissement* in which it was situated, and he tried in vain to explain it away. Probably he was ignorant of the facts and jealous of the repeated flattering references to the thesis of his junior. He objected to Depaul's policy of smaller lying-in hospitals on the remarkable ground that "Cette mesure . . . serait très préjudiciable a l'enseignement de l'obstétricie."

Cazeaux recognised only a primitive alteration of the blood, not a consequence. His opinions on most points were just like those of Dubois and Danyau. In times of epidemics there is in addition some mysterious influence the nature of which we do not know.

Bouillaud is faithful to his banner of *organicien localisateur.* He regards puerperal fever as an inflammatory malady modified by the special condition of pregnancy: it is in no way distinct.

Dubois, in a second address on the 27th of April, said that, like his colleagues Danyau and Depaul, he taught the doctrine of contagion, but with further evidence he was prepared to modify the import of the term. But in this address he merely criticised: he stated no evidence of his own. He referred to the Doctrine of Semmelweis "relating to the accidental propagation of puerperal fever by presumed inoculations" in these terms: "Received in England with an ardour and a conviction which fortify my doubts as to the real importance of a certain number of documents relating to the propagation of puerperal fever by effluvia, this theory is probably forgotten even in the school where it was born. . . . I regard the conditions proper to the development of puerperal fever as pre-existent to labour in a certain number of cases . . . . it is nothing but an epidemic which brings to the Maternité and to the Clinique women pregnant or in labour who already exhibit the most characteristic and usually the most serious symptoms of puerperal fever."

At the next meeting Dubois addressed himself to the question of suppressing the lying-in hospitals, a policy which was receiving support in influential quarters at the time, and of course he expressed a preference for their improvement.

Coming to the disconcerting statistics of Tarnier, he tried to throw doubt upon their accuracy. This portion of his oration is mere quibbling: even if Tarnier had made some mistakes, the general effect of his figures after making every allowance still remained appalling. Dubois, owing to his great official position, had to review the discussion and express a pious wish that it might be beneficial to obstetric science.

Velpean took part in the discussion late, but his contribution was one of the few which could be considered of any value. He reviewed some early work on puerperal fever which he had been engaged upon forty years before, in which he sought to prove that many diseases began with an alteration of the blood, and that the pus which produced puerperal fever could kill with or without local lesions. As far as puerperal fever was concerned, there were two parties present in this discussion; one was essentialist and the other regarded the disease as symptomatic. He took the side of the latter. Puerperal fever is a disease primarily local, accompanied by various lesions. . . . The lesion of most frequent occurrence is peritonitis complicated in various ways. . . . The element of puerperality does not make this a malady apart of which the basis dominates the form, of which the gravity is not measured by the extent of the inflammation, but which depends on a special element . . . which dominates it. . .

Velpean then discussed the question of overcrowding as an etiological factor, and considered the influence attributed to it as not proved.

Contagion is an element more delicate to appreciate. He would not like to admit what Arneth (Semmelweis) says about it, nor the opinion of Simpson who thinks that puerperal fever can be propagated by the medical practitioner or by the midwife. . . . He could find no proof in favour of essentiality. He believed that in metritis, phlebitis, and perimetritis we often had the results of traumatism of the genital organs during labour. There was nothing specific about it.

The remaining meetings were occupied almost entirely with replies, and produced nothing new.

This discussion by "*les maitres français les plus autorisés*" was followed by a less formal and even more prolonged debate in medical circles, and in the medical press of Paris and provincial France. The report of the discussion in the Academy was translated into German and other languages, and the proceedings created more or less interest over the greater part of Europe.

But the debate, as was to have been expected from the first, proved barren of results; it only demonstrated the phenomenal diversity of opinion which then existed in Paris on everything relating to puerperal fever.

In the course of the voluminous press correspondence and articles which followed the discussion, Dr. Auber, the author of a treatise on Medical Philosophy, summed up the discussion in this way: "Among the thirteen

Academicians whom we have heard, we can count essentialists, demi-essentialists, essentialists against their will, essentialists without their knowledge, absolute localisers, half and quarter localisers, localisers with a leaning towards essentialisation, and essentialists with a love of localisation, specificists, typhists, traumatists and neo-traumatists."*

This description was probably intended to be more witty than exact, but it conveys to us some impression of the chaotic difference of opinion on such a serious subject as puerperal fever among the most prominent obstetric 'authorities' in France during the Second Empire.

Each "authority" appeared to retain his former opinions as is usual on such occasions, and there appeared to be no sign of any progressive change. As Herrgott says: "The medical press which criticised the discussion presented itself a spectacle of the diversity of opinion and of the practical sterility which was the result: "*La boussole manquait complètement sur cette mer agitée hérissée de récifs.*"

Semmelweis was stirred up to wrath and contempt to an unusual degree when the report of the proceedings of the Paris Academy of Medicine reached him in Buda-Pesth. Concerning the discussion he wrote in the *Ætiologie*†: The dry straw which was then exposed to the light we shall leave unthrashed; grain cannot be beaten out of it; let it suffice to quote from Dubois first mentioning his scientific position as described by Arneth: "In Obstetrics the medical profession forms no republic: one individual appears to be supreme. Before him all flags are lowered. His opinion is sought for whenever any fresh phenomenon makes its appearance. Men, who themselves have accomplished much, declare that their work is the fruit of his teaching. . . . Even in the Academy men listen breathless to his words; through his influence opinions borrowed from abroad and hitherto strongly opposed by his fellow-countrymen obtain naturalisation in France (*das französische Bürgerrecht*).

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* *Herrgott* III. p. 295.

† p. 457.

Let us hear now how Dubois, the *summum forum obstetricium* of France condemns my opinions. He says: "Also the theory of Semmelweis . . . received in Germany and England with such lively interest has not been proved correct, and it is probably by this time forgotten in the school where it originated."

After still further quotation from Dubois Semmelweis exclaims: "And by this person without a conscience is French midwifery dominated! Poor Humanity, to whom dost thou entrust thy life!

"My *Lehre* is not already forgotten in the school where it originated, and what is more it will not be forgotten in the future. . . . My Doctrine is only slandered in the school where it originated, but my Doctrine avenges itself as all things noble avenge themselves on their slanderers. . . . That my Doctrine has not accomplished more, my traducers will have to answer. . . . And this revenge gives me this weapon into my hand that I can declare to my antagonists in the school whence my Doctrine emanated: "Your own diminished mortality is the strongest contradiction of your professions!"

TARNIER. If Semmelweis had possessed more intimate knowledge of medico-social and scientific matters in Paris and could have read the signs of the times he would have been less exasperated. The old order which made it possible for Dubois, father and son between them, to occupy the most important official teaching appointments in Midwifery in Paris for already over half a century, was coming to an end, and the future reformer both in scientific midwifery and in hospital administration was already at work.

Tarnier was destined to become the scientific successor of Semmelweis. There is a curious analogy in their circumstances and their early predelictions, and a remarkable similarity in the influences at work upon the minds of the two men, and their consequent pursuits and achievements at an interval of ten years.

We have seen that Tarnier's thesis had been referred to in complimentary terms by some of the speakers in the great debate, including Dubois himself. That thesis was entitled "*Recherches sur l'état puerpéral et les maladies des femmes en couches.*" When this thesis had served its purpose, Tarnier lost no time in publishing a treatise entitled "*La fièvre puerpérale observée à l'hospice de la Maternité,*" in which he assumed a firmer and more independent tone.

Tarnier as a young graduate entered the Maternité early in 1856. Later in one of his important works he tells about the spectacle which was there presented to his horrified vision. It would be hard to say which had the more shocking experience, Semmelweis or Tarnier. "In the course of this year there occurred at the Maternité 2,237 cases of labour with 132 deaths; that is to say 1 patient in 19 died, nearly 6 per cent. The mortality was not equally distributed throughout the months of the years. On some days and during some weeks it was enormous. . . . They sometimes died in fact at the average rate of five per day. . . . From the 1st of May to the 10th there were 32 cases of labour, and we registered 31 deaths!

It was then decided to close the Maternité, but this radical measure was adopted rather late, for the malady had already carried off 64 of the 347 women confined from the 1st of April to the 10th of May. . . . To a sympathetic and imaginative young man, it was a shocking, a heart-breaking spectacle.

"I made inquiries of my masters at the Maternité, Dubois, Danyau, Delpech. I said to them that it appeared to me impossible that such a mortality could be general, and that it could prevail equally in private practice. They assured me that the same condition of things prevailed in the city, and that the epidemic was then as severe there as in the hospital. "That has always been the case,'' they said, '' and it will be always so.''

''The fatalism of such an answer was revolting, and with the ardour and confidence of youth I attempted to discover the cause of epidemics of puerperal fever, and the means of preventing them.''

Tarnier, like Semmelweis, was struck with the sterility of the pathological anatomy accepted then in explanation of puerperal fever, and in face of the doctrinal anarchy which universally reigned, he set resolutely to work by a new method.

First of all he endeavoured to find if it was true that puerperal fever ravaged the whole of Paris, and particularly if it raged around the Maternité the same as in the hospital itself. After prolonged and patient investigation he made out that the mortality from puerperal fever in the *arrondissement* in which the Maternité hospital was situated was only the seventeenth part of that of the hospital. Tarnier demonstrated scientifically, mathematically, that the famous *genius epidemicus* could not climb over the walls of the Maternité.

A few years later the same fact was proved to be true of all the lying-in hospitals of Europe by Tarnier's friend, Leon Le Fort.*

Five years is a long time for a mere student of medicine, and Tarnier did not know then about the work of Semmelweis, and the episode of Arneth's address to the *Académie de Médecine de Paris* in January, 1851, on the means proposed by Semmelweis to prevent puerperal fever epidemics in the lying-in hospital of Vienna, was probably unknown to him. Even if the subject had been mentioned to Tarnier later, when he could understand it, he would have only heard the disdainful misrepresentations of Dubois, the *Summum forum obstetricium,* and of Danyau and other obstetric oracles who formed the Committee that never reported on Arneth's address, and who actually did mislead him in the relative incidence of puerperal fever in the city and in the Maternité. "But," says Pinard,* "let us acknowledge with infinite sadness that it was the routine tradition to generally ignore the discovery of Semmelweis."

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* *Des Maternités.*

It is however hardly possible to believe that an earnest student and investigator like Tarnier, hearing the sneers about Semmelweis even in the Academy discussion, would not inquire independently and receive some knowledge and inspiration.

Tarnier then, without knowing exactly about the Semmelweis discovery, worked along the same lines, in the same scientific spirit and inspired by the same humane desires and aspirations, and he ultimately reached practically the same conclusions. His propositions stated in the Thesis were: 1. Puerperal fever is contagious. 2. Puerperal fever is the result of blood-poisoning. 3. In place of the term puerperal fever we would prefer that of *Septicémie puerpérale.* 4. It is a veritable poisoning comparable to that of the dissecting room. 5. When the poisoning is violent death follows rapidly and the poison leaves no traces behind it. 6. That there is an alteration in the blood cannot be doubted, but in what it consists it is impossible to say.

The thesis concludes as follows: "We have not invented the facts; we have not manipulated them to support a preconceived idea; we have discussed them with impartiality according to the principles of pathology, and it is with sincerity and conviction that we maintain that puerperal fever is contagious."

By the term "contagious" Tarnier obviously means the same as the *Uebertragbarkeit* of Semmelweis, so that the conclusions are practically identical.

Animated with this sincerity and conviction Tarnier went to work with the ardour of an apostle, and with the tenacity and courage which give rise to high hopes and produce great results.

On the very day in July, 1858, on which the great debate on Puerperal Fever concluded Tarnier published a work on *La Fièvre puerpérale observée à l'hospice de la Maternité*. It had evidently been kept back till after the discussion out of respect for his superior officers. Tarnier was as zealous and firm in propagating his opinions as Semmelweis; there was no moral cowardice about him; but he was more prudent and worldly-wise. He conciliated men in influential positions, and promotion came rapidly. There was opposition of course to the doctrine of the contagious, that is infectious, origin of puerperal fever by men in official positions who had during their professional careers missed the import of the malady, especially by Dubois. Tarnier says later: "In spite of the opposition made me by my master it was to the demonstration of this truth, until then misunderstood in France, that I consecrated the greatest part of my Thesis."

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* *Annales de Gynécologie et d'Obstetrique.* January, 1909.

Tarnier became Surgeon-in-chief to the Maternité in 1867, and from that position he continued to urge upon the authorities the adoption of measures of prevention, chief among which was the isolation of infected puerperæ; but it was not until 1871 that he succeeded in attaining his object. The puerperal fever mortality of the institution was then 9·3 per cent. but we should keep in mind the wretched condition of the patients during the war and the Siege of Paris.

The constituted authorities had been difficult to move; they had for years resisted overwhelming evidence. In the years 1861—1862 for example 1,169 women out of 14,199 died in the hospital; in the city of Paris 559 lying-in women died out of 99,991 confined. Tarnier exclaimed over the report: "1,090 women in two years or 545 per annum struck down to death in the hospitals who would have been probably spared if they had been confined in the city. These figures go beyond all probability: one hesitates to write them down. Such a mortality becomes a public calamity, and it ought to disappear from the day that it is known."

Tarnier had said in his Thesis that the cause and nature of the blood-poisoning was still unknown, but there were men then at work in the same city of Paris who were beginning to solve the enigma.

Pasteur was thinking and working: he had begun to publish his earliest results, but the proclamation of his matured and specially relevant opinions was to come in a few years, and the fruits were to be gathered in time.

Pinard says: I do not forget the words of Lister who said in his opening address in 1869: "The germ theory is the polar star which ought to guide us safely in a navigation which, but for it, would be desperately difficult."

In Pinard's article, repeatedly quoted, there is a remarkably interesting and impressive table showing the results of Tarnier's methods during four successive periods up to the present time. The only explanation required to those who do not know medical Paris is that the Maternité, the school for midwives, and the Clinique Baudelocque, for students of medicine, are one great lying-in hospital within the same enclosure, and that the Clinique Baudelocque is a comparatively recent foundation.

PERIODS.	MORTALITY.	
1. Period of inaction (1856–69)	9·31%	Maternité.
2. Period of isolation (1870–80)	2·32%	
3. Period of antisepsis (1889–98)	0·67%	Clinique Baudelocque.
4. Period of antisepsis and asepsis (1899–08)	0·29%	

Tarnier's great work, which is entitled to rank near the *Ætiologie* of Semmelweis, was *De l'asepsie et de l'antisepsie en Obstétrique*. But with him puerperal fever never engrossed all his thoughts and sympathies, and as is generally known he performed other great and original service to Obstetrics.

Silberschmidt.

Towards the end of 1859 when Semmelweis was well advanced in the preparation of *Die Ætiologie,* and was probably actually engaged upon the controversies of the " Opinions," there appeared a treatise or monograph entitled: " Historical-critical Exposition of the Pathology of Puerperal Fever from the most ancient times up to our own." The author was Dr. Silberschmidt, assistant to Scanzoni at Würzburg. In what may perhaps have been a "stop press" commentary on this work Semmelweis says*: " Upon this work was bestowed the prize of the Faculty of Medicine of Würzberg. The author of this work speaks against my opinions as to the origin of puerperal fever, and since this work was awarded a prize (Preise gekrönt) by a Corporation of which Scanzoni is a member, and to him in fact in such matters is conceded a position of decisive influence, it is more than certain that we harboured an illusion when we hoped that Scanzoni had come over to our opinions. . . ."

But let us hear what Silberschmidt has to say. He says: " Skoda and Semmelewis believed that the most important cause of puerperal fever is cadaveric poison . . . So even the first sentence proves that Dr. Silberschmidt presumes to pronounce judgment upon my views which he has not succeeded in understanding." Semmelweis in reply explains the "three sources" of which the most active in the First Obstetric Clinic of Vienna was cadaveric poison. . . .

. . . If Dr. Silberschmidt is capable of forming an opinion of his own, and not merely an amanuensis who lets any one wheedle and flatter him into a course of action, let him thoroughly study this treatise and he will form other conclusions for himself. . . .

Semmelweis goes on taking this impertinent understudy too seriously, doubtless quite correctly assuming that it is to Scanzoni he is replying. His opponent recalls what has been written against Semmelweis and passes over in silence the writings of his supporters, and the success of his method of prophylaxis in Vienna and Buda-Pesth. It is disingenuous work and in other respects such as a self-respecting man could not have put his hands to. "Silberschmidt has compiled this contribution not as a thoughtful investigator but as a writing-machine." He supports the views of Kiwisch about puerperal fever epidemics, and he copies Scanzoni's pathology of puerperal fever—hyporinosis, pyæmia, and blood-dissolution including the exclusion of even fatal metritis and endometritis from the category of puerperal fever. The whole production is a stupid ill-conditioned attack by a sycophant for which his master must be held largely responsible. Curiously enough the award of a prize to this essay proves that the author's backers, Scanzoni and Company, did not know even the German literature of their own subject, or that they connived at plagiarism.

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* *Aetiologie* p. 403.

v. Winckel* in his admirable summary of the history of puerperal fever says: "Eisenmann (1837) was the first to attempt the exposition of the various theories of puerperal fever: his work is the prize-essay of Dr. Silberschmidt (1859) who copies him almost verbally in very many passages (an sehr vielen Stellen fast wörtlich gefolgt)."

Returning later to Silberschmidt and Scanzoni Semmelweis says: "Silberschmidt had to prove that my opinions about the causation of puerperal fever are erroneous. In order to prove this Silberschmidt calls attention to the want of success of chlorine disinfection employed by Scanzoni at Prague, and also to the unfavourable observations of Kiwisch at Würzburg. . . . If my opinions are true then is the Scanzoni pathology of puerperal fever colossal nonsense. . . . This pathology must be presented to the world as the flower of the endeavour of centuries, and whatever stands in the way of this design must receive no mercy, not even the Truth itself. . . . Du lieber Gott, when will puerperal fever cease to spread over whole provinces when by means of such unconscientious disingenuous opposition the medical practitioners scattered over whole provinces are befooled."

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* *Die Pathologie und Therapie des Wochenbetts* (1866).

PREPARATION OF THE "ÆTIOLOGIE" . . . OF PUERPERAL FEVER, 1857–60.

It was in the autumn of 1857 that Semmelweis finally resolved to prepare and publish a book on the subject which occupied nearly all his thoughts and was the chief interest of his life. He began by compiling the vast amount of statistical material at his disposal. Well might Fritsch express his appreciation in terms already quoted: "Die vernichtende Logik seiner Statistik"[1]; the words just lay hold of the most striking feature of the *"Ætiologie."* Another remarkable feature is the intimate knowledge of the writings on puerperal fever of all countries and periods, especially of the records of the subject in the United Kingdom. It is humiliating for a British member of the medical profession to read the opinion of an English medical writer[2] stated in spite of the overwhelming available evidence to the contrary, so late as 1875, that Semmelweis "gives proofs of possessing very limited knowledge indeed of the literature of his subject, and we cannot feel surprised that he finds but little difficulty in disposing of his opponents to his own satisfaction." When we find the same writer committing himself to the opinion that "as an observer of pathological conditions he appears to have been decidedly inferior to Gordon" we can afford to ignore the misleading statements from first to last. We only mention it in illustration of the simple credulity and ignorance of English visitors to Vienna in Carl Braun's time.

The biographical portion of *Die Ætiologie* is scanty and disappointing, like the autobiographies of so many distinguished men of all nations, the most remarkable example being probably the *Vita sua* of George Buchanan, "the only man of genius his country ever produced" according to Dr. Samuel Johnson.

Semmelweis says in the introduction: "The object of this Treatise is to set forth historically the observations made in the Clinic, and to explain how I began to doubt the truth of the prevalent teaching with regard to Puerperal Fever. . . . It may be considered a proof of my aversion to polemics that I have left unanswered so many attacks, but I believed that I could leave the truth to open the way for itself. After waiting for thirteen years I find that the amount of progress has not been made which is necessary for the welfare of mankind. . . .

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[1] *Grundzüge der Pathologie und Therapie des Kindbettfieber* (1884).

[2] *Brit. Med. Journal*, March, 1875.

"To this disinclination for every thing polemical there is added an inborn dislike for everything that can be called writing (*gegen Alles was schreiben heisst*).

"But fate has selected me as the champion of the Truth which is expounded in this Treatise, and there is a duty laid upon me which I cannot refuse to perform."

The account of the elimination of alleged factors and of the discovery itself is of engrossing interest, and it is also of permanent value to the medical profession throughout the world as showing the advantages of philosophic doubt, and of the application of logical method in the endeavour to ascertain the causes of the phenomena of disease. Much of the controversial matter might have been omitted without disadvantage, but it is also interesting biographically and psychologically in exhibiting the mental and emotional changes produced in the writer by neglect, misrepresentation and slights amounting in his mind to persecution. It has the drawback of preserving the names and controversial methods of some infinitely little men among his official professional contemporaries which would have been better consigned to oblivion.

The work as a whole was written in the German language but considerable portions of it were published during the course of preparation in Hungarian in the "Orvosi hetilap." One of the most interesting of these articles published in 1858 was entitled: "The Difference between my opinions and those of English Obstetricians on the Etiology of Puerperal Fever."

This article is perhaps the most important of all those published separately and then interpolated in the *Ætiologie*.

During the preparation of the work Semmelweis wrote a large number of letters to teachers of midwifery all over Europe. He read all references to his Doctrine which appeared in the literature of Europe whether in formal manuals, in periodicals, or in the annual summaries of the contributions to midwifery and diseases of women which appeared in the medical journals; and he let little escape his notice and animadversions.

When anything more important than usual appeared, he wrote a criticism and arrested the ordinary progress of his book in order to interpolate a special article, not always in the right place, and seldom with any special heading to intimate to the reader that he was entering upon more or less of a digression.

From all directions came tidings of the influence of what he and his friends had already published on the etiology and prophylaxis of puerperal fever, but he was seldom cheered by a friendly article or an appreciative address in the proceedings of an Obstetrical Society. The methods of controversy were hateful and contemptible. Professorial contributors must attribute the outbreaks of puerperal fever in their Clinics to any or every cause except that which was declared by Semmelweis to be the only real cause, contact with decomposed animal organic matter, and the paltriness of some leaders among the opponents such as Braun in Vienna was occasionally exhibited in omitting even the name of Semmelweis while criticising his doctrine.

A letter from Professor Dietl of Cracow,[1] in answer to the inquiries of Semmelweis in 1858, sheds a dreary light on the state of opinion and some methods prevalent among teachers at that time. "In the course of my travels I observed that appreciation of your views on the origin of puerperal fever found expression in the arrangements of some lying-in hospitals. . . . Members of the staffs avoided contact with the cadaver, as in Copenhagen. . . .

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[1] *Aetiologie* . . . p. 306.

"Direct inquiries addressed to the Directors of these institutions should obtain for you much of the information which you desire.

"Upon the whole one hears less about those devastating epidemics of puerperal fever. . . .

"Perhaps the cause of this lies in the observance of the methods of carrying on the practice of midwifery based upon your experience without any public admission of the change. For the purposes of investigating the truth on this point a journey round the world would be well worth your while."

Well may v. Waldheim (p. 142) declare: They envied this man his celebrity. The vanity of the learned considered it ridiculous that this simple person who spoke a dialect in his scientific addresses and contributions to debate, who had never published a single scientific contribution to obstetrics, could have made such a discovery. The thing must be simply ignored. "Professors who were not even specialists in obstetrics had most improperly puffed (*ungebührlich aufgebauscht*) the discovery. There might be something in it, but Semmelweis had only given attention to one of the causes of puerperal fever which was already well known to the medical profession in England, one cause among thousands. In order to combat the other alleged causes they excogitated or borrowed all possible hygienic measures, and attributed the more favourable results to them. Even those who started as opponents out of sheer vanity and dislike, finally became convinced antagonists and believed what they alleged."

Such was the atmosphere of alternating extremes of emotion resulting from the intelligence received from abroad concerning the spread of his Doctrine that the work continued to grow. The author was at the same time occupied in his routine professional labours both private and public, and no doubt many an hour was lost owing to distraction caused in such an excitable individual by the crosses and disappointments which he met with in his endeavour to secure improved accomodation and arrangements in his Clinic against official opposition or indifference.

Removal of the Obstetric Clinic.

In spite of all efforts it was not till early in 1860 that the Obstetric Clinic of the University of Pesth was removed to the outside of the city along with the other hospitals.

Here then was a partial reward for all the restless endeavours of years. From the time of his return to Buda-Pesth in 1850 Semmelweis had been advocating in season and out of season, in every way within his reach, the adoption of the methods and the amenities of civilization which he knew to be prevalent in Western Europe. The measure of his success is indicated in an article published in the *Wiener medicinische Wochenschrift* in April, 1860.

" If it is not to be denied that the institutions have obtained the advantages of more room, it must be admitted that the internal fittings (furniture, beds, etc.) are in the old wretched condition; the broken tables and the ragged and worn-out bedclothes, all brought from the old hospitals. Especially the Obstetric Clinic is in an undescribably pitiable condition; there poor lying-in women are to be found some of them partly on straw spread on the floor, some of them on wooden benches, others crouching in any corner of the room, weary and worn-out: only to few is it vouchsafed to find a regular bed on which to stretch their weary limbs. Everywhere you find dirty bed-linen with bedclothes old and worn, and almost in rags. It will be admitted that the professor of this Clinic cannot be blamed for this evil condition of things (*Ubelstände*) because of laziness or indifference."

VI.

PUBLICATION OF "DIE ÆTIOLOGIE."

Although the Clinic was improved in some respects it was still very defective. It was again placed on the second floor with the Surgery Clinic immediately beneath it, and the space was still so limited that no room or ward could be reserved for isolation.

Still by his watchfulness and by the example of keen interest in the work, and of conscientiousness in the performance of his duties, Semmelweis had now around him a specially intelligent and loyal staff of assistants and nurses upon whom he could rely to carry out his instructions with exactness and punctuality. Consequently in spite of all the drawbacks with regard to space and the construction of the Clinic, he had only five fatal cases of puerperal fever among 520 patients in the School-year 1859–60, a mortality of 0·9 per cent.

Semmelweis worried about the number of cases of self-infection of which he had a definition of his own: but his was not quite the self-infection which some teachers of midwifery in the twentieth century still appear to be believe in. Then again he attached too much importance to the atmosphere as a carrier of infection, and though by his watchfulness and attention to details he overcame the real dangers and difficulties, the dread of non-existent dangers was continually with him and kept him restless and worried.

What the want of isolation accommodation implied is brought out in the *Nachtrag* which is a sort of afterthought in a few short paragraphs added on at the end of the *Ætiologie*. The chief relevant facts with regard to difficulties in the Clinic were, that mixed up with the lying-in women were gynæcological cases, and that patients died of tuberculosis pulmonum and typhoid fever, and that during the same School-year, 58 students of medicine and 199 pupil-midwives received instruction in clinical midwifery within the narrow limits of the Clinic.

Semmelweis worked very hard at his book in 1859 and 1860, and he added to his professional literary burdens the editing of the portion of the medical journal devoted to obstetrics and gynæcology.

One day in 1860 he met his friend Dr. Hirschler in the street, greeted him in a great state of excitement and marched him off home with him. There he read to Hirschler the Introduction to his book. He considered the work now complete; he had finished with the Preface (*Vorwort*). Yet it was not complete: Semmelweis was continually writing fresh chapters, all in a great hurry, constantly repeating portions without co-ordination, and hurrying the manuscript off to the printer without revision.

In this manner was written "*Die Ætiologie, der Begriff und die Prophylaxis des Kindbettfiebers*": the book bears abundant internal evidence of the method of its creation.

It was finished in August, and published in October, 1860. It is a work running to 543 pages. It has been long out of print, and was becoming almost unobtainable, when Hungarian patriotism made it accessible to all. In 1905 the whole works of Semmelweis were edited by Tiberius von Gyory, privat-dozent in the University of Buda-Pesth, and published under the auspices of the Hungarian Academy of Sciences in an admirably printed volume of 600 pages.

The first portion of the book contains nearly all implied in the title of the whole, and yet it has no heading to convey to the expectant reader what subjects are to be discussed in its pages. We have already given in summary the contents of this very important portion.

Then follows the "Import of Child-bed Fever"

(Begriff des Kindbettfiebers) which contains only about a dozen pages.

"Ætiologie" which follows is the central and important part of the work.

"Prophylaxis" does not make a long chapter and its importance is lessened by much that has been already stated in the previous parts of the book.

"Opinions and Correspondence" forms a large part of the work. It is entirely controversial and deals with the opinions which had reached the Author up to the time of publication.

THE IMPORT OF CHILD-BED FEVER.
(*Begriff des Kindbettfiebers.*)

In this division of his book Semmelweis expresses concisely his conception of puerperal fever, but he devotes many unnecessary pages to emphasizing his opinions by repetitions of what he had already written in the course of fifteen years on three different institutions, under Prophylaxis and Correspondence.

It will suffice therefore to select passages and summarize.

"Supported by the experiences which I have collected in the course of fifteen years in three different institutions all of which were visited (heimgesucht) from time to time by puerperal fever to a serious extent, I maintain that puerperal fever, without the exception of a single case, is a resorption fever produced by the resorption of a decomposed animal organic material. The first result of this resorption is a blood-dissolution (Blutentmischung); and exudations result from the blood-dissolution.

The decomposed animal organic material which produces child-bed fever is, in the overwhelming majority of cases, brought to the individual from without, and that is the infection from without; these are the cases which represent child-bed fever epidemics; these are the cases which can be prevented.

In rare cases the decomposed animal matter which when absorbed causes child-bed fever, is produced within the limits of the affected organism. These are the cases of self-infection, and these cases cannot all be prevented.

The source whence the decomposed animal organic material is derived from without is the cadaver of any age, of either sex, without regard to the antecedent disease, without regard to the fact whether the dead body is that of a puerperal or non-puerperal woman. Only the degree of putrefaction (*Fäulniss*) of the cadaver has to be taken into consideration. . . .

The sources of the decomposed animal organic material which, conveyed to the individual from without, causes puerperal fever, are all diseases whatever the age or sex if only the disease in its progress produces a decomposed animal organic material, without regard to the fact whether the patient suffered from puerperal fever or not: only the decomposed animal organic material as a disease-product has to be taken into consideration.

The case of medullary cancer of the uterus and the foul-smelling knee-joint are once more mentioned in illustration.

"In the lying-in portion of the St. Rochus Hospital at Pesth it was the most heterogeneous surgical diseases whose putrid products gave rise to child-bed fever."

The sources whence is derived the decomposed animal organic material which, when brought to the individual from without, produces puerperal fever, are all physiological animal organic structures which when withdrawn from the laws of vitality have undergone a certain degree of decomposition. What the object actually represents is of no importance; it is the degree of putridity which has to be considered.

. . . . At the Obstetric Clinic of the Faculty of Medicine at Pesth, it was physiologic human blood and normal lochia which were the etiological factor of the puerperal fever, inasmuch as they were left for a long time soaking the bed-linen and undergoing decomposition.

"The carrier of the decomposed animal organic material is the examining finger, the operating hand, the bed-clothes, the atmospheric air, sponges, the hands of midwives and nurses which come into contact with the excrementa of sick lying-in women or other patients, and then come again into contact with the genitals of women in labour or just confined; in a word the carrier of the decomposed animal organic material is everything which can be rendered unclean by such material and then come into contact with the genitals of the patient."

The site of infection by the decomposed animal organic material is the internal os uteri and upwards from there. The inner surface of the uterus . . . is robbed of its mucosa and presents an area where absorption occurs with extreme readiness (ungemein resorbtionsfähige Flache). The other parts of the mucosa are well clad with epithelium and do not absorb unless they are wounded. If it is injured any portion of the genitals becomes capable of absorption.

With regard to the time of infection, it seldom occurs during pregnancy because of the inaccessibility of the inner absorbing surface of the uterus by reason of the closure of the os internum. In cases in which the internal os uteri is open during pregnancy infection may occur then, but these cases are rare because there is seldom any need for passing the finger within the cervix uteri.

"I neglected to take notes of the cases in which puerperal fever began during pregnancy at the First Obstetric Clinic of Vienna but I believe it to be near the truth if I put down the number of cases as about twenty. By puerperal infection the pregnancy was always interrupted . . ."

The time within which infection most frequently occurs is during the stage of dilatation. This is owing to frequent examinations made with the object of ascertaining the position of the fœtus.

A proof of this is that before the introduction of chlorine disinfection nearly all the patients after labour, protracted in the dilatation period, died of puerperal fever.

Infection seldom takes place during the expulsion stage because the surface of the uterus cannot then be reached.

In the third stage, or after-birth period, and during the puerperium, the inner surface of the uterus is accessible, and at this time especially, the atmospheric air loaded with decomposed animal organic material may gain access to the internal genitals and set up infection." Reference is again made here to the curious knee-joint case of 1847.

" In the after-birth period and during lying-in, the infection may be produced by the bed-linen coming into contact with the genitals which have been injured in the process of parturition. . . . "

Self-infection: The decomposed animal organic material which when absorbed brings on puerperal fever is in rare cases not conveyed to the individual from without but originates within the affected individual owing to the retention of organic material which should have been expelled in child-bed. Before its expulsion decomposition has already begun, and when absorption occurs puerperal fever is produced by Self-infection. These organic materials are the lochia, remnants of decidua, blood coagula which are retained within the cavity of the uterus. Or the decomposed animal organic material is the product of a pathological process, for example, the result of a forcible use of the midwifery forceps causing gangrene of bruised portions of the genital organs and consequent child-bed fever by Self-infection.

" When we declare that child-bed fever is a resorption fever in which as the result of absorption a blood-poisoning occurs, and then exudation follows, we do not imply that puerperal fever is peculiar to the lying-in woman and restricted in its incidence to lying-in women. We have met with the disease in pregnant women and in new-born infants without regard to sex. This is the disease which was fatal in the case of Kolletschka; and we find it affecting anatomists, surgeons, and patients who have undergone surgical operations.

"Puerperal fever is therefore not a species of disease: puerperal fever is a variety of Pyæmia.

"With the expression pyæmia different meanings are bound up: it is therefore necessary to explain what I mean by pyæmia. I understand by pyæmia a blood-poisoning, produced by a decomposed animal-organic matter.

"A variety of pyæmia I call child-bed fever, because special forms of it occur in the genital sphere of pregnant parturient and puerperal women . . . .

"Puerperal fever is not a contagious disease. By contagious disease we understand the sort of disease which itself produces the contagion by which it is propagated, and this contagion again produces in another individual the same disease. Smallpox is a contagious disease because smallpox produces the contagion by which smallpox can be reproduced in another individual. Smallpox produces in another individual smallpox and no other disease . . . For example, a person suffering from scarlet fever cannot cause smallpox in another individual.

"Such is not the position with childbed fever: this disease can be produced in a healthy normal puerpera by a disease which is not puerperal fever . . .

"Puerperal fever is not conveyed to a healthy puerpera unless a decomposed animal-organic material is carried to her. For example, a patient becomes seriously ill with puerperal fever, and when this puerperal fever runs its course without the production of a decomposed animal-organic matter, which appears externally, then is the disease not conveyable to a healthy normal puerpera. But when puerperal fever runs its course in such a way as to produce a decomposed matter appearing externally, then is childbed fever capable of being conveyed (*übertragbar*) to a normal healthy puerpera. For example, a puerpera is suffering from the malady in the form of septic endometritis . . . from such a patient is puerperal fever capable of being carried (*übertragbar*).

Hence it is that a controversy has arisen among the contagionists owing to the non-recognition of these two classes of cases. No final decision could be arrived at because the contagionists could quote cases in which the spread of puerperal fever from a sick to a normal puerpera could not be denied; and the opponents of the contagion theory could also quote cases in which the spread of the disease had not occurred when it must have occurred if the disease were contagious.

"Puerperal fever is not a contagious disease, but puerperal fever is conveyable from a sick to a sound puerpera by means of a decomposed animal organic material.

"After death the body of every lying-in woman becomes a source of decomposed material which may produce puerperal fever; in the cadaver of the puerpera we consider only the degree of putrefaction. When we have reflected that the overwhelming majority of cases of puerperal fever are produced by infection from outside, and that these cases can be prevented, and that in only a small minority of cases puerperal fever is the result of unavoidable self-infection, the question arises: if all fatal cases, not resulting from puerperal fever, and if all cases of infection from without are prevented by suitable measures, how many lying in women die as the consequence of self-infection?

"It is not possible to answer this question for want of statistics, and we must attain complete control of material and environment so as to banish conveyed infection from our hospitals before we can obtain reliable statistics of self-infection."

Then follows a laborious effort to analyse the statistics of the Vienna Lying-in Hospital before the anatomical trend of medical science, that is from 1784 till the time when Boër was dismissed and Klein came on as successor, and assuming that under Boër the mortality was almost entirely from self-infection, an attempt is made to obtain the actual amount of self-infection now prevalent. The whole theory was founded on error, and though the reader must admire the patient industry with which Table xviii. was compiled, and the logical reasoning founded on the premisses assumed to be true, he cannot be surprised at the unsatisfactory conclusion. The problem of the proportion of fatal cases of puerperal fever from self-infection to those from conveyed infection is not solved. It was insoluble then and remains so still.

Towards the end of *Der Begriff* the author returns to the mortality of the three lying-in hospitals with which he has been connected, and leaves the case of each in a rather indefinite position: he succeeds in conveying to the reader the unsettled condition of his own mind on the relation of self-infection and conveyed infection.

"At the Obstetric Clinic of Pesth, I lost in the first year of my official work, out of 514 patients, 2 from puerperal fever. In the second year, out of 551 lying-in women, 16 died from puerperal fever. In the third year, out of 449 lying-in women, 18 died from puerperal fever. The high mortality of both these years was the consequence of infection from without by means of filthy bed-linen."

ETIOLOGY.

After some recapitulation of what has been stated under *Begriff* Semmelweis proceeds: This is the place for subjecting to criticism the hitherto accepted Etiology of puerperal fever in as far it maintains the bringing from without of a decomposed animal organic material: this is the place to test how far the decomposed animal organic material is made to originate in the individual herself. . . .

All that part of the hitherto prevailing etiology of childbed fever which includes neither the bringing of a decomposed animal organic material from without nor the production of such a material within the individual, all that portion of the hitherto accepted etiology of childbed fever we shall not recognise as an etiological factor in puerperal fever.

It is at the present time the most widely prevailing opinion in the medical profession that puerperal fever consists in a blood poisoning, and that the anatomical results of puerperal fever are only the expression of efforts to eliminate the poison from the blood. In this opinion I also concur.

Among the causes which produce this blood-poisoning, men blame epidemic and endemic influences, emotional conditions, errors of diet, chilling, etc. My contention is that the blood-poisoning, without the exception of a single case, is produced through the absorption of a decomposed animal organic material which is either conveyed to the individual from without . . . or which has originated in the affected individual herself, cases of self-infection.

Armed and furnished with this conviction we shall now proceed to a criticism and estimate of the hitherto prevailing Etiology of puerperal fever. . . .

We shall begin with the epidemic influences and give expression to our unshakeable conviction (*unerschütterliche Uberzeugung*) that there are no epidemic influences capable of producing puerperal fever, that there never have been such epidemic causes of puerperal fever, and that the endless series of epidemics which are recorded in medical literature, were all preventible cases of infection from without, that is to say, that every one of the cases was caused by the conveyance to the individual from without of a decomposed animal organic material.

The grounds for the confidence and courage with which I assail a belief several centuries old are the following:

Before everything stands the unshakeable rock on which I have raised the edifice of my *Lehre* concerning puerperal fever, the *factum,* that owing to the measures which I have adopted and carried out from May, 1847, to the present day, 19th April, 1859, ... at three different institutions which used to be afflicted yearly with frightful so-called puerperal fever epidemics, I have brought about a condition of things in which a case is met with only now and again. Even the most stubborn defender of the epidemic theory of puerperal fever could hardly call this state of matters an epidemic. And when occasionally the number of fatal cases has increased, it could always be proved that the more numerous fatal cases were not caused by epidemic, that is to say, atmospheric, cosmic, telluric influences, but that they were always the result of conveying a decomposed animal organic material to the individual owing to breaches of my rules and injunctions. If puerperal fever were produced by atmospheric, cosmic, telluric influence it could not be prevented, and behind this unavoidability the epidemicists entrench themselves and disavow responsibility for the ravages of puerperal fever, and they do nothing to diminish its incidence.

" I admit that I would be helpless in face of such influences, but nevertheless if I succeed in preventing this disease which is declared to be unpreventable I produce proof that the disease does not depend upon unpreventable atmospheric cosmic telluric influences and I demonstrate that the disease results from a removable (*entfernbar*) cause; and that removable cause is decomposed animal organic material. . . .

It was the endeavour to prevent the access of such material to individuals entrusted to my care, that brought success in the reduction of the mortality, not because I had found the secret of making epidemic influences innocuous. . . .

The sickening and dying of many individuals from the same disease within a definite time does not complete all that is implied in an epidemic, else would every battle be an epidemic, for in a battle many individuals die from the same cause in a definite period of time. . . .

Semmelweis next discusses the question whether puerperal fever occurs like real epidemic diseases at some definite season of the year, and he proceeds to prove by statistics that puerperal fever is not bound up with any season in particular.

He gives a Table (No. XIX.) compiled with vast industry in order to prove that over a long series of years there was no month or season showing a regular maximum or a regular minimum mortality. This is a return to the subject with which he dealt in an earlier part of his book. Semmelweis had alleged that the high mortality in Vienna coincided with the time when students were busiest with dissection and with operations on the cadaver, and he endeavours once more to support that thesis. He gives figures to prove that in January, 1849, the mortality was 2·25 per cent., whereas in January, 1842, it was 20·84 per cent. In February, 1848, the minimum mortality for that month was 0·68, and in 1846 occurred the maximum for February, viz., 18 per cent., and so all through all the months of the year. In December we find the minimum mortality for that month fell in 1848 with 1·34 per cent. and the maximum in 1842 was 31·38 per cent. . . .

It is the prevailing opinion that Winter is the season of the year which specially favours an outbreak of puerperal fever, and as a matter of fact we must admit the evidence that, upon the whole, in the winter months there is frequently a less favourable condition, and seldom a more favourable, and in Summer time there is frequently a more favourable condition of health, and less frequently an unfavourable.

The explanation of Semmelweis is something like this: But these phenomena are not to be explained by the atmospheric influences of Winter, else puerperal fever would never occur in the more severe and extensive forms in the Summer time. The phenomena are to be explained by the occupation of those who attend the lying-in hospitals, and that occupation depends upon the season of the year.

After the long vacation in August and September, the students return to their studies with fresh zeal and industry, and as far as midwifery is concerned, there is such a rush to the Lying-in Hospital in the Winter months that many have to wait their turn for a long time before they have the opportunity of attending cases.

In the Summer time, on the other hand, from half to two-thirds of the places were usually unoccupied. In the winter season the students were all busy with pathological anatomy and medico-legal post-mortem examinations, while students attending the midwifery clinic were also fully engaged in the Medical and Surgical departments of the General Hospital.

In the Summer time all these occupations were to a large extent neglected. The beautiful country surrounding Vienna had greater attractions for students than the foul-smelling dead-house or the sultry wards of the hospital . . . The cold and darkness of Winter and the heat of Summer in turn have their effects on the relative time of dissecting and visiting the patients in the lying-in hospital, and all the arrangements are productive of puerperal infection when the students are numerous, but in Summer they are comparatively few.

Was there no Winter in Vienna during the twenty-five years when the mortality in the Lying-in Hospital averaged less than 1 per cent?

In the Winter of 1847-48 and 1848-49, there was no epidemic as a result of the chlorine disinfection.

At the St. Rochus hospital in Pesth, midwifery cases were admitted only in August and September, yet every year it was ravaged by a puerperal fever "epidemic."

After calling attention to the fact that there are lying-in hospitals in all climates, and that these are impartially visited by so-called epidemics of puerperal fever, the incidence of puerperal fever in teaching institutions, and the sparing of those lying-in hospitals where midwives only are admitted for training; the introduction of decomposed matter where no teaching at all is carried on, as for example in the paying department of the Vienna Lying-in Hospital, which is hermetically sealed to all medical men except its own staff, and where none but cases of self-infection might occur instead of actually an average mortality of 2·5 per cent.

The explanation of the incidence of childbed fever in the paying department is that the chief medical officers were both working at gynæcology in the General Hospital; then there were 600 to 800 autopsies for medico-legal purposes in the General Hospital, and these medical officers had to conduct these autopsies between them.

Is the unfavourable condition of the health of the women confined in the paying division still an enigma?

Lying-in hospitals which are at the same time teaching institutions shew more unfavourable results than those hospitals which are not schools of midwifery.

And among teaching institutions the best results are obtained by those which are specially reserved for the instruction of midwives. The reason of this is that midwives are not to anything like the same extent as medical men employed upon things which bring their hands into contact with decomposed matter. . . .

An apparent exception to this rule is to be found in the *Maternité* of Paris, which is exclusively devoted to the training of midwives, yet has as high a mortality as Dubois' *Clinique* in Paris which is reserved for the teaching of students of medicine.

This statement is made on the authority of Arneth who was an eye-witness of the state of things which he described in his book.

Semmelweis compares the *Maternité* and the *Clinique* of Dubois in great detail, and he goes into the history of the *Maternité* in order to explain the deplorable results obtained there for a generation and more. The matter is all fairly relevant to the argument on etiology, but it is too voluminous, and the statistics may be convincing, but they are not attractive reading. For us the conclusions are sufficient.

In the first place, we learn by the tables that for a long series of years the mortality at the Maternité had been on the average of 4·18 per cent., and at Dubois' *Clinique* 4·55 percent.

In the Paris Maternité the system of instruction is so arranged that the midwives are as much occupied with work which renders their hands foul with decomposed matter as medical students are elsewhere.

Semmelweis draws chiefly upon Osiander,[1] who visited Paris about fifty years before, and owing to the friendship of Baudelocque obtained special facilities for making observations.

The Director of the Maternité was rather proud of his system of teaching, and yet Osiander found the pupils writing notes of the cases, and copying the expressions and details from one another although the cases were altogether different! This has its parallel in practices said to be prevalent in the junior schools in certain Spanish-American countries where a class of small boys are set the task of writing out the confession of their sins, and, to save the trouble of thinking, copy points from one another's manuscripts.

Osiander's first episode probably only made his German readers smile over the vaunted superiority of the training of French midwives, when it was used as an argument for keeping medical men out of the practice of midwifery, but he describes proceedings of the gravest import bearing on the question of the etiology of puerperal fever.

"The female pupils (midwives) usually attend the post-mortem examinations . . . which take place close to the Lying-in Hospital. I have often witnessed with astonishment the lively interest with which young women took part in the cutting up (zerfleischen) of the bodies, how they with bare and bloody arms and with large knives in their hands, amidst squabbling and laughter, cut out the bony pelvis to make preparations for themselves."

In eleven years—1798 to 1809—there were 17,308 confinements at the Maternité, 2,000 of the women became seriously ill, and 700 died and were "secirt."

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[1] Bemerkungen über die französische Geburtshilfe, nebst einer ausführlichen Beschreibung der Maternité in Paris. Hannover, 1813.

Osiander speaks of peritonitis (*Unterleibsentzündung*) as synonymous with puerperal fever; he also says: "The disease is observed especially in the winter months;" and the mortality between 1803 and 1808 was so shocking that the authorities practically suppressed the details. . . . "It was the difference in the mortality of the two divisions of the Vienna Lying-in Hospital which raised the first doubts in my mind concerning the doctrine of epidemic puerperal fever." The same inequality in the mortality of two divisions of the same institution we find in Strassburg under exactly similar circumstances. For details reference is again made to Dr. Arneth's book.

There were formerly two professors at Strassburg, and "the Lying-in Hospital consists of two parts: *la Clinique* for medical students: and *le Service* or School for Midwives. The two portions were separated by only a thin partition. . . . It was not possible to obtain exact information about the mortality, but both professors were agreed that it was constantly higher in the *Clinique* than in the *Service*." Semmelweis wrote to Professor Stoltz and to Dr. Wieger on this subject in 1858. Wieger, among other statements, wrote: "What Arneth told you is quite true."

Professor Stoltz replied at considerable length in French; in the course of his letter he said, "le fait est exact;" and he went on to describe the defects of his institution, and to say nice complimentary things to Semmelweis. He also promised to introduce his method of prophylaxis into the whole of the Strassburg *Maternité*.

Semmelweis proceeds to discuss the question of puerperal fever at Strassburg at considerable length with quotations from Arneth's book; he then returns to Osiander and the Paris *Maternité* and compares its high mortality with that of Vienna in the same years, where it was about 1 per cent.

We next find again a reference to the introduction of the anatomical basis of all medical studies into the Vienna School, with a summary of the history of the Lying-in Hospital from 1784, including the mortality from puerperal fever up to the introduction of chlorine disinfection. After Vienna we come upon a distinctly interesting reference to the practice of the Obstetric Clinic of the University of Buda-Pesth.

It was in the forties that the anatomical direction was given to the study of medicine in Buda-Pesth.

" My predecessor, Hofrath Birly, formerly assistant to Boër, believed that the more favourable results obtained at Buda-Pesth, compared with the less favourable results in Vienna, depended upon the free use of purgatives, because in his opinion puerperal fever was produced by an unclean condition of the *primæ viæ,* and he delivered annually a full-dress philippic against Vienna, declaring that the high mortality of the Lying-in Hospital there was the result of neglect of purgatives.

" But as soon as medicine in Buda-Pesth assumed the anatomical direction, the purgatives lost their prophylactic virtue, and the *Professorencollegium* on one occasion, before I had the honour to be a member of it, had officially demanded the closing of the Obstetric Clinic even during the School-year.

" I cannot supply figures because the notes were lost during the Revolution . . . . but the facts are not contestable " (p. 137).

We have next several pages of statistics and repetition of statements with regard to the production of puerperal fever by conveyed infection, not by epidemic influence, with only an occasional graphic detail which arrests attention.

" My successor as assistant, Carl Braun, has written against my opinion. Carl Braun's successor, his brother Gustav, demonstrated what opinion he held concerning the origin of childbed fever by his 400 deaths in the year 1854. This mortality has been exceeded only three times in the whole seventy-five years of the history of the Vienna Lying-in Hospital."

After more pages of statistics and analysis and exposition, we reach the conclusion once more that not only in Vienna but in every lying-in hospital everywhere else in Europe, the mortality from puerperal fever does not depend upon any epidemic influence, but upon a decomposed animal organic material brought from without into contact with the genitals of the individual patient.

This conclusion is brought forcibly home by reviewing the practices and results of the lying-in hospitals of Great Britain and Ireland, and comparing them with the less favourable health conditions of puerperæ in the lying-in hospitals of Germany and France.

There is no evidence whatever that the atmospheric influences in Great Britain differ in any respect from the continental, but the opinions of the medical profession in the United Kingdom differ essentially regarding the origin of puerperal fever from the opinions entertained in France and Germany.

The medical profession in England regard puerperal fever as contagious; in France and Germany the prevailing opinion has always been that puerperal fever is not contagious. That puerperal fever is not contagious is also my belief. . . .

" But puerperal fever is conveyable (übertragbar) from a sick pregnant, parturient or puerperal woman to a healthy pregnant, parturient or puerperal woman by means of a decomposed material produced by the sick pregnant, parturient, or puerperal woman. Puerperal fever is not conveyable during life from every sick pregnant, parturient or puerperal woman to a healthy individual, but only from those infected women who produce a decomposed material. After death puerperal fever is conveyable from every cadaver of a puerpera to a healthy individual when the cadaver has reached the necessary degree of decomposition."

English practitioners, starting with the conviction that puerperal fever is contagious, do not visit a healthy pregnant, parturient or puerperal woman when they have paid a visit to an infected pregnant, parturient or puerperal patient, without previously disinfecting their hands with chlorine disinfectant and without changing their clothes; and when the number of puerperal fever cases increases in their practice, they go away from home or completely abandon midwifery practice for a time. The English practitioner, if he must undertake the postmortem examination of a patient who has died from puerperal fever, never visits a normal parturient or puerperal woman without first pushing the same precautions to the fullest extent.

In every case in which the infected puerpera produces a decomposed material, the English practitioners do something which is superfluous but not harmful; they destroy the decomposed material in the belief that they are destroying a contagium which would cause puerperal fever if carried to a healthy parturient or puerperal woman.

After the postmortem examination of a patient who has died of puerperal fever, they take similar precautions with the object of destroying the contagium, that is, the decomposed material with which their hands have been rendered unclean.

German and French practitioners, believing that puerperal fever is not contagious, and not knowing that the malady may be conveyed by means of a decomposed material, take part in performing post-mortem examinations of women who have died from puerperal fever, and they visit puerperal fever cases even when these are producing a decomposed matter, and then without any antiseptic precautions they at once visit healthy parturient and puerperal women. In this way they carry to their patients decomposed matter which when absorbed causes puerperal fever.

In English lying-in hospitals therefore all those cases which depend upon contact with the puerperal cadaver or sick puerpera are eliminated, and the consequence is a more favourable condition of the health of the patients in the English lying-in hospitals, where puerperal fever is believed to be contagious.

How it happens that from the various sources numerous cases of infection may arise, Chiari gives some good illustrations in a contribution published in 1855. As one of the incidents at the Prague Lying-in Hospital, it is recorded that a woman suffering from *endometritis septica* died in the Lying-in Hospital, and that all the nine patients in the same ward, with only one exception, died in a few days. Septic endometritis was one of the diseases which Scanzoni excluded from the category of puerperal fever.

Semmelweis was evidently so pleased with Chiari's article that he incorporated it verbally in the *Ætiologie*.

In order still more completely to prove that a better state of health invariably exists in lying-in hospitals where the opinion prevails that puerperal fever is due to a contagium, and measures are applied with the object of destroying the contagium, Semmelweis draws largely upon a work just published by Professor Levy of Copenhagen, and translated into German by Michaelis of Kiel. Levy's subject was "The Lying-in Hospitals and the Teaching of Midwifery in London and Dublin," and his material was chiefly obtained in a recent visit to the chief towns of the United Kingdom.

In the course of the introduction to the translation of Levy's work Michaelis writes: "We must feel it our duty to thank our English colleagues for their example of fruitful endeavour, for the hope which we may now entertain of a happier future."

In this portion of the Etiology Semmelweis draws to a large extent upon the work of Levy supplementing his matter by quotations from the book of his friend Arneth,[1] who devoted a *Wanderjahr* in 1850–51 to visiting the lying-in hospitals of France and the United

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[1] Ueber Geburtshilfe u. Gynækologie in Frankreich, Grossbritannien und Irland, Wien, 1853.

Kingdom. Many pages are devoted to the statistics of the British lying-in hospitals, with some account of the history of each, and of the individual members of the staffs who had written on the subject of puerperal fever. Whenever an illustration is required in order to compare methods and results we are brought back to Vienna or or Paris or Buda-Pesth.

There is occasionally a digression of a controversial kind, such as his analysis and apparent annihilation of Litzmann's[1] loose assertions. Semmelweis says: "In order to avoid repetitions I have selected this place in my treatise for an exposition of these circumstances, although I am speaking of other things for the moment." This is an unnecessary explanation: the digressions without obvious justification are so frequent, and repetitions occur everywhere.

After the chiefly statistical account of British lying-in hospitals, Semmelweis, making free use of Arneth's work, proceeds with a highly instructive compilation of English experience regarding the incidence of puerperal fever outside the lying-in hospitals owing to the conveyance of a decomposed material. This consists of purely clinical observations by general practitioners of medicine, a thing unheard of in Continental Europe. From the middle of the eighteenth century onwards with increasing volume such evidence brought English obstetricians to the conviction that puerperal was the work of a contagion, an entity which with proper precautions could be destroyed. Between the generally accepted Continental theory of a *genius epidemicus,* whose logical result was *laisser faire,* and the British theory of contagion there was all the practical difference between apathetic fatalism on the one hand, and strenuous hopeful and largely successful exertion on the other.

Arneth commences the portion of his work on which Semmelweis eagerly lays hold, as the only matter available for his purpose in Europe, with the observations

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1. Das Kindbettfieber in nosologischer, geschichtlicher und therapeutischer Beziehung, Halle, 1844.

and the results obtained by general practitioners, with this paragraph (v. Arneth, p. 334):

"Puerperal fever is such a frightful malady that it must interest us in the highest degree to learn what the English medical practitioners think of it, generally speaking, and especially what is the generally accepted opinion about that enigmatical subject, the etiology, and also their opinion about what is the best treatment for the disease."

Following Arneth for facts, and interpolating opinions of his own, Semmelweis tells the story of the accumulation of evidence by English practitioners which appeared to them to establish the theory of the contagious nature of puerperal fever. We follow in his pages the stories of Roberton, of Manchester, of the inquiry of Storrs among his neighbours, and his conclusions from the evidence brought to his knowledge, and the experience more or less important of Reedal, of Sheffield, Sleight, of Hull, and all the others whose contributions are familiar to the English reader of the history of midwifery. All of these may be more readily referred to in English medical literature.

Semmelweis brings out with disconcerting clearness the curious English theory of the relationship between puerperal fever and scarlatina and erysipelas and all the zymotic diseases, a confusion which in England, on the authority mainly of Barnes, remained more or less prevalent to nearly the end of the century. On this subject he states, then, what is the universal opinion now: "Puerperal fever stands to erysipelas and its sequelæ in no other relation than to any other disease which produces a decomposed material. . . . When English medical men recognise only puerperal fever and erysipelas as the sources of the decomposed material which produces puerperal fever, they draw the boundaries much too narrow . . . . puerperal fever is the same disease as that of which surgeons and anatomists and patients who have undergone surgical operations may be the victims . . .

"The clothes of the medical practitioner do not come into contact with the genitals of the patient, so that the habit of English accoucheurs in changing their clothes, so as not to spread the fever, is a harmless but superfluous precaution." . . . In order that puerperal fever may arise, it is *conditio sine qua non* that the decomposed matter comes into contact with the genitals, therefore all possible examinations are devoid of danger to the patient except the *exploratio obstetrica interna.*

There is something pathetic in the proof of this statement: it consists in a reference to the results of the year's work at the First Obstetric Clinic of Vienna in 1848 which are evidently accepted as bordering on perfection: "I and the students in Vienna in 1848 never changed our clothing after being occupied with things possessed of properties which made them capable of producing childbed fever; we only thoroughly disinfected our hands by chlorine washing, and in the year 1848 we lost only 45 patients out of 3,556, that is, in the proportion of 1·27 per cent.!" Compared with the past and the succeeding ten years this result might well seem the highest possible success. Yet in the same hospital the mortality is to-day only a twentieth part of that which gave Semmelweis such satisfaction.

There is next a long and tedious recapitulation of all the arguments already employed to disprove the old doctrine of epidemic influences in the production of puerperal fever.

"I cannot believe that any man who is in earnest about the truth can believe in the doctrine of epidemic puerperal fever, except up to the moment when the want of harmony between the doctrine and the data are made clear to him. The man who, in spite of the data still professes to believe in epidemic puerperal fever, has not the courage to stand up for the truth . . . the man who, in spite of the data, still actually believes in epidemic puerperal fever . . . has no capacity for understanding and reasoning; he carries about with him only words learned by note and stored in his memory."

"The doctrine of epidemic puerperal fever explains something unknown by that which is also unknown."

Such are the grounds of my convictions; I wish in the interests of humanity that all who are interested in the questions relating to puerperal fever could form the same convictions with me.

Here then we have a vast mass of evidence stated in a clear and convincing manner. It seems to us now absolutely convincing and irresistible. And it was convincing to the younger and unprejudiced men, according to the evidence of Fritsch and Hugenberger and others. How it came to be resisted in favour of mere unintelligible formulæ by the contemporary professors of midwifery makes one of the most remarkable chapters in the psychological history of medicine. We see in it the consequences of importing emotional bias into the discussion of purely scientific questions.

The chapter headed the "Endemic Causes of Puerperal Fever" consists of a discussion of the real relation of certain alleged factors to the etiology of puerperal fever, but with the exception of a large number of new tables and some minor points which do not add to the force of the argument, there is no fresh matter. The place for this chapter is early in the work, where the discovery of the true etiology is discussed and proved by a process of elimination of alleged causal factors.

Semmelweis rejected overcrowding of a lying-in hospital as an etiological factor of any importance, and thereby damaged his case in the opinion of Western obstetricians. He was logically, and in fact practically right, by reason of the conditions and qualifications attached to the broad statement of his opinion, but misrepresentation was made easy; qualifying statements could be so readily omitted.

"Overfilling of lying-in hospitals is only conditionally an endemic factor in the production of puerperal fever, inasmuch as in an overfilled lying-in hospital it is more difficult to maintain the requisite standard of cleanliness, inasmuch as in an overfilled hospital it is more difficult to completely isolate patients who may be a danger to the others; in this way can overfilling possibly become the cause of the production of a decomposed matter, and thus indirectly overfilling may lead to the conveyance of decomposed matter from one individual to another. But when, in spite of overfilling, the necessary degree of cleanliness is observed so that no decomposed matter is produced . . . under such conditions it is a matter of indifference to the lying-in hospital patient whether the hospital is overfilled or not " (p. 213).

This opinion is elaborated and supported by a great mass of statistics which occupy the greater portion of the chapter.

The only point of any importance which is not mere repetition is the discussion of puerperal miasma. The argument amounts to this, that when the air of a lying-in room is so loaded with exhalations from the skin of the patients, from secretion of milk and from the lochia, and from the emanations from the new-born, in the absence of ventilation, it may form a decomposed matter and become itself the conveyer of decomposed matter to the genitals, and thereby the producer of puerperal fever. "When this is the meaning attached to puerperal miasma I agree in accepting it. Anything else over and above this as puerperal miasma does not exist. On this import of puerperal miasma is based the due employment of means of ventilation, and the practice of isolation of affected cases."

PROPHYLAXIS.

Inasmuch as the only cause of puerperal fever is the bringing to the individual of a decomposed animal organic material from without or the production of such deleterious matter within the individual, the task of prophylaxis of puerperal fever must consist in preventing the access of decomposed material from without, the arrest of the development of such material within the organism, and the removal as quickly as possible from the organism of such a material so as to prevent its reabsorption and thereby the occurrence of puerperal fever.

It is easier to prevent the fingers from being rendered unclean by decomposed material than to wash them thoroughly clean after contact, therefore all students of medicine should be forbidden by law to engage in such work as must soil their fingers with such material during the time when they are occupied with the practical study of midwifery. . . .

If the Semmelweis teaching is made a mock of by professors of midwifery among their students, what amount of conscientious disinfection can be expected from the students? When death has obtained a rich booty, then is the failure of the chlorine disinfection declared to be a proof of the epidemic origin of puerperal fever.

Then follows an appeal to all civil authorities everywhere to make illegal the practice of midwifery at lying-in hospitals simultaneously with work at pathology and operations on the cadaver (p. 268).

This opinion is emphasized by an exposition of the injurious consequences of practical courses in midwifery by operations on the cadaver, then and for many years after boasted of as a sort of proud distinction of the Vienna school. All pathological anatomy and even surgical work in the curriculum should be finished before the practice of midwifery is begun. These opinions appear now to be mere platitude with our modern opinions founded on long experience, but at that time in Continental Europe he who proclaimed them was as one crying in the wilderness.

Besides keeping the hands clean, is was necessary to institute rigorous precautions with regard to all articles which were made unclean with decomposed material and then were brought into contact with the genitals, such as instruments, bed-linen, sponges, bed-pans, etc.

The conveyer of the decomposed matter may also be the atmospheric air. Hence free ventilation is necessary to carry away from the lying-in wards the exhalations from patients before they can form a puerperal miasma.

It is also essential for the welfare of patients of a lying-in hospital that several rooms for isolation be provided so as to promptly separate infected from normal puerperæ.

It does not matter how many lying-in women are nursed in one room provided "when the number of lying-in women is in due proportion to the size of the room."

Small lying-in hospitals have no advantage over large hospitals where proper precautions are taken. This point is illustrated by the history of the small hospital at Würzburg where Kiwisch, an epidemicist, lost 26·5 per cent. in one year from puerperal fever, whereas in the vast lying-in hospital of Vienna the highest mortality ever reached was 15·75 per cent. in 1842.

The paragraphs devoted to the prophylaxis of self-infection might stand to-day in any text-book of practical midwifery containing the opinions and advice of an experienced obstetrician. "If after all a decomposed material has actually been produced in the individual then it must be at once got rid of by cleanliness and injections so as to prevent resorption as far as possible."

"Whoever practices this prophylaxis will experience the pleasure, not from time to time to lose every third or every fourth patient from puerperal fever, but perhaps to lose only one in four hundred, certainly not more than one in a hundred" (p. 272).

Correspondence and Opinions in the Literature for and against my Doctrine.

"If this treatise had no other object than to establish our Doctrine on an unshakable foundation, and to make perfectly clear the sad error of the epidemic theory of puerperal fever, if this only was our object, we might suitably bring our treatise to a close here.

"But that alone cannot be the object of this treatise, for my Doctrine is not firmly established in order that the book expounding it may moulder in the dust of a library : my Doctrine has a mission, and that is to bring blessings into practical social life. My Doctrine is produced in order that it may be disseminated by teachers of midwifery, until all who practise medicine, down to the last village doctor and the last village midwife, may act according to its principles; my Doctrine is produced in order to banish the terror from the lying-in hospitals, to preserve the wife to the husband, the mother to the child. . . . If now after twelve years since the birthday of my Doctrine we put the question : has this doctrine fulfilled its mission, the answer is very depressing.''

''The essentials of the Doctrine have been proclaimed all these years and the truth has been demonstrated, and seeing the object of the practice founded on the theory is the saving of human lives, one would have expected that men of science would have reflected and taken action. All are agreed as to the terrible nature of the malady; we ought to have been equally unanimous in treating it in the manner which promised the highest success.

''But experience has taught us differently; the vast majority of medical lecture-rooms still re-echo with lectures about epidemic puerperal fever, and with philippics against my Doctrine, consequently one generation after another of infectors are sent forth into the professional practice of their lives, and it is impossible to foresee the time when the last village doctor and the last village midwife will cease to carry infection. . . .

'' In recent medical writings my Doctrine is either ignored or offensively assailed . . . and Directors of lying-in hospitals might be mentioned who apply my method and yet attack it, and ascribe their success to other causes.

''Indignation at the greatness of this scandal has thrust the pen into my unwilling hand. I think it would be criminal behaviour on my part if I were longer to remain silent, and neglect producing unbiassed, impartial, and complete evidence in favour of the practical extension of my Doctrine."

Such is the introduction to the history of the controversy over the "*Ætiologie.*"

Here then we have made an attempt to convey to the English reader some impression of the contents of one of the greatest medical works of the nineteenth century, together with some idea of the manner in which the new Doctrine was conveyed. The subject was too great and serious for the high professional rhetorical style; the only element of rhetoric in it is the rhetorical artifice of re-iteration, of which there is superabundance. The story is always told in a straight, clear and earnest and dignified manner, the style occasionally rising to eloquence; but there is nothing artificial about it; it is perfectly natural and singularly suitable to the immeasurable importance of the message.

Semmelweis had brought a new idea into the world fraught with immense consequences to humanity. For him it was the summit of the truth *Die Höhe der Wahrheit,* the "eternally true" doctrine of the cause of one of the direst calamities to human society, and a revelation of the means of preventing its continuance. The message had to be effectively and persuasively conveyed so that clear conviction might produce strong action. And granted the faith created, the action should be easy, for the doctrine was vastly too simple for belief. In place of all the systems of belief built up for generations, the professional world was asked to believe that a decomposed animal organic matter, directly conveyed to the genital tract, was the only cause of puerperal fever, no case of puerperal fever excepted since the human female began to bear children. This is the universal belief of the medical profession to-day, and recognised to be eternally true, because the author had discovered the true principle on which to found his doctrine, and principles do not change. Small details and temporary fashions and vogues have been laid stress on or come into prominence at times by the influence of strong personalities;

but they have all accorded with the fundamental principle.

We have in a former chapter given some account of the chaotic doctrine of the etiology of puerperal fever before Semmelweis came upon the scene. Now in spite of the overwhelming evidence in support of the simple doctrine of the consequences of contact with decomposed animal organic matter, and the preventive influence of a small piece of a common chemical substance, we shall find that cosmos was not to be so easily created out of this chaos.

The new idea had to be assimilated by the professional mind throughout Christendom, and the process was disappointingly slow; in many quarters the organs of assimilation revolted against the task.

SPREAD OF THE DOCTRINE AFTER PUBLICATION OF DIE ÆTIOLOGIE.

We have traced the reception given to the Doctrine since its first proclamation in Vienna in 1847 until the publication of *Die Ætiologie* in 1860, and we must now proceed to observe the progress made by the *Lehre* throughout Europe during the life of Semmelweis and in the generation immediately following the publication. But the distinction of before and after publication is largely arbitrary. Since the first announcement by Hebra in 1847 in a not unimportant medical journal, the published report by Haller, the reception and publication in London and Edinburgh and Dublin, it was a discreditable thing for any official teacher of midwifery in Europe to remain ignorant of the new doctrine. But accepting the painful historic facts, we must recall the chief incidents in the spread of the Doctrine after the publication.

Among the first of his duties after publication Semmelweis considered it to be the presentation of copies of his work to certain medical societies and to personal friends. To the United Kingdom were despatched copies to Routh, Copland, Simpson, and Murphy, and others.

To the Academy of the Sciences of Buda-Pesth he presented a copy accompanied by a letter written in the Hungarian language. In the course of the letter he said: " Through the favour of divine providence it has been granted to me to discover the true character of this malady which has hitherto been considered epidemic, and on the basis of that discovery to prevent the occurrence of the disease, which is the chief consideration. . . . Fate so ordained that this discovery was made during the time when, as assistant, I was residing in the Vienna Lying-in Hospital far from my Fatherland. This is the reason why my discovery was first communicated to the German colleagues. Since my return home I have laid my experience before the Hungarian brethren and published my theories concerning puerperal fever in the *Orvosi Hetilap*.

" Whilst my *Lehre* met with no opposition here, it was much attacked and misrepresented in Germany.

" My duty to humanity therefore requires me once more to explain my theories, and to demonstrate the insignificance of the objections raised against them."

Semmelweis received many friendly letters in acknowledgment of the gift of his book, and he quotes some of them in the " Open Letter to all the Professors of Midwifery." The letter of Dr. Kugelmann of Hanover must have given him much pleasure. Kugelmann had been a student of v. Siebold's at Göttingen, and he had also worked under Michaelis at Kiel, so Semmelweis found in him a sympathetic reader. In the course of his letter Kugelmann said: " Permit me further . . . to express the holy joy (heilige Freude) with which I studied your work, *Die Ætiologie*. . . . In the course of a conversation on the subject with a colleague here, I felt myself compelled to declare: This man is a second Jenner; may his services receive a similar recognition and his efforts bring him the enjoyment of a similar satisfaction. . . ."

Kugelmann happened to possess a copy of Jenner's work with his autograph; it had been a presentation copy from the author to Blumenbach of Göttingen, and he begged Semmelweis to accept it "as a mark of my unlimited respect."

In a second letter Kugelmann says: "It has been vouchsafed to very few to confer great and permanent benefits upon mankind, and with few exceptions the world has crucified and burned its benefactors. . . .

"I hope you will not grow weary in the honourable fight which still remains before you. . . ."

Letters direct or indirect from Dommes of Hanover, Prof. Pernice of Greifswald, and Pippingskjöld, of Helsingfors, complete the list referred to by Semmelweis.

Bruck says that the publication of *Die Ætiologie* hardly attracted any attention. In the medical press appeared only a few short notices, some of them by no means complimentary. An exception among the special journals was *Froriep's Notizen,* which spoke of the Semmelweis discovery as the most important progressive step in medical science of modern times.

Markusovszky wrote a friendly appreciation in the *Orvosi Hetilap,* and Fleischer published a complete summary in another journal, but the Hungarian language was little read in the West of Europe, and the articles of Markusovszky were of comparatively little service to the spread of the *Lehre.*

BREISKY. Among the earliest and most important of the unfavourable criticisms was the article by Breisky, an assistant at the Lying-in Hospital of Prague. This article is largely quoted by Bruck as typical of the state of opinion among obstetricians of eminence at the time when the *Ætiologie* appeared. The critic takes exception first of all to the self-consciousness of Semmelweis's method of addressing the leading men whom he names in the *Stimme.* After years of silence he proclaims his discovery as the Koran of a puerperal creed which he preaches with fanatical zeal and enters into battle with threatenings of fire and sword for the unbelievers. Breisky is very sarcastic about the discoverer of the " eternally true " etiology of puerperal fever. In his opinion Semmelweis has not succeeded in producing anything like an exact and complete demonstration of the truth of his *Lehre*. He has not proved the identity of pyæmia and puerperal fever. Breisky then enters upon a destructive criticism of all the evidence brought forward by Semmelweis for the three lying-in hospitals to which he so often refers. Even infection by want of cleanliness of bedding, etc., at the St. Rochus Hospital at Buda-Pesth is called in question. Hardly any obstetrician except Semmelweis has observed any advantage from the method of prophylaxis recommended in the book, and the conclusion is therefore reached that cadaveric infection cannot be the cause of puerperal fever even in such cases as those described.

Breisky then gives some more of the statistics almost characteristic of Prague, and concludes further that the attributes of the deleterious matter to which Semmelweis pins his faith do not account for the facts.," This is the fate which the etiology of childbed fever shares with that of so many pathological processes. There is the 'something' which is yet unknown in the etiology and has still to be discovered."

Then comes in some quotation from Dr. Charles West, as the typical medical philosopher, about the *divinum aliquid*, the *το δε̂ιον* of Hippocrates, and the attempts that have been made in vain to read the riddle of Nature. This is the sort of pseudo-philosophic verbiage, much in vogue then among medical writers, and employed to conceal poverty of thought, and inability to generalise on facts.

It is simply deplorable to find a young and able man like Breisky giving expression to such sentiments and opinions. He might have found it impossible to resist pressure from his chiefs, amounting to compulsion, to write the notice about Semmelweis in an unfavourable sense, as was to be expected at Prague; but nothing could justify the stupidity and cruelty of the smart jibes and sarcasms. The subject was vastly too serious for such treatment. He would have done better to imitate in a becoming manner the style of Semmelweis.

Breisky's article is chiefly of interest as expressing the almost unanimous opinion of the German "authorities" in obstetrics at that time. For them Semmelweis was still, as he had been misrepresented for fourteen or fifteen years to be, the "Apostle of Cadaveric Infection." [1] They would not learn at first-hand the full import of the *Lehre*.

And one point more about this Prague Lying-in Hospital where the staff had always been partisans against Semmelweis: let us hear the independent testimony of Le Fort,[2] most patient, impartial, and clear-eyed of observers, who made a professional journey round the lying-in hospitals of Europe two or three years later. "In spite of its favourable position outside the city and its isolation the Maternité of Prague has always had a rather high mortality. Epidemics are somewhat frequent, and if in the statistics the mortality is not more on the average than 4 per cent., we must keep in mind that a rather large number (un assez grand nombre) of women affected with puerperal fever are transferred to the general hospital which diminishes to a remarkable extent the mortality of the Maternité. The impression which was made upon me by the Clinical Obstetric Section was most unfavourable. . . . We found there many women very ill or dying of puerperal fever amidst normal puerperæ. . . . I should be glad to hear of the demolition of this establishment. . . .

"Destruction of the present Maternité of Prague appeared the only means of improvement, because it cannot be destroyed without first erecting a new institution."

It is a remarkable phenomenon in the history of midwifery that men placed in positions of grave responsibility could be found straining the truth by the publication of Prague statistics, and sinning against the clearest light by resisting evidence which all the world since then has accepted as overwhelming.

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[1] Bruck, p. 84.

[2] Le Fort : Des Maternités, 1864.

Bruck, in commenting on this article of Breisky's, says that he devotes space to it because it reflects the opinions of the vast majority of obstetric specialists at the times it was published (die überwiegende Anzahl der Fachmänner). The belief in authority with regard to the etiology of puerperal fever was at its zenith; the younger men swore by whatever the men of recognised importance pronounced as true, and naturally the great men who had committed themselves to an unfavourable opinion the *Lehre* delayed as long as possible to admit their error. Bruck thinks that Semmelweis would have done better for the spread of his doctrine if he had built for the eminent professors a golden bridge. We much question it. One thing certain is that they did not study his book, and for them Semmelweis remained "the apostle of cadaveric poison, the preacher of a one-sided creed."

Markusovszky. The criticism of the *Ætiologie* by Breisky of Prague was not such as to bring any satisfaction to Semmelweis or his friends and supporters. As emanating from Prague it could not be expected to be generous, but it was distinctly prejudiced and unfair. Within a few years the constant struggle for the spread of the Doctrine and the painful disappointments which it had brought him had now made Semmelweis bitter in spirit and irascible in temper; it had also produced a physical change, an appearance of weariness and of ageing rapidly, which was readily observed and commented on by all who knew him.

Markusovszky took upon himself the task of replying to Breisky in the "Orvosi Hetilap," and consequently Breisky remained practically unanswered. The powerful article of Markusovszky, written in the Hungarian language, remained unknown to Western Europe for many years, until it was translated and published by Bruck.[1]

Markusovszky wrote: " The investigation into the etiology of puerperal fever is not yet complete; so much is certain. It would not be complete even if the source of infection of puerperal fever were universally recognised to be due to decomposed animal matter. Even the definition of the disease which Semmelweis employed must be considered provisional, inasmuch as the elements entering into it—the import of pyæmia, of absorption and of fever—still form subjects for research.

In our opinion it has still to be demonstrated by a process of exact investigation what is the nature of that organic material which produces the infection, and what are its histological and chemical relations . . . in what particular manner it obtains access to the organism: what manner of chemical and physiological changes are thereby produced; what are the conditions of resorption, and of its active influence since it does not seem to occur in all cases; what is the nature of the physiological processes by the combination of which puerperal fever occurs sometimes without any exudation in one case, and in another case with the accompaniments of extensive exudation and metastases. . . . . All these questions must be cleared up, and that is a task for the obstetricians to perform? And are we to draw the inference that because the new doctrine has not as yet been explained in every detail that it is therefore false, and that the ancient definiteness of epidemic darkness is to be preferred?

When we consider how much the opinions about pyæmia have changed in our own days, how the knowledge of embolism, in spite of the progress of pathological anatomy, is an achievement of only the most recent date . . . we certainly cannot demand from the relatively new knowledge, and from a single individual, the solution of all the questions to which answers can only be obtained with the aid of all the branches of natural science. . . . Breisky has made no reference to the life-saving discovery and the favourable results already obtained: he has only thought it right to raise a series of objections of doubtful value, and to demand from the obstetrician such a full and complete development of his Doctrine in all directions as can be reached only by the co-operation of physiology, chemistry, physics and histology, when in the course of time these branches of science have made further advances."

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[1] Bruck, Semmelweis, p. 93.

Such is in essentials the far-seeing and sagacious article of Markusovszky's on the objections to the Semmelweis discovery. It was friendly, but it required courage and patience with his friend, who was sadly excited and unreasonable in his reception of the truth contained in the article. He appears to have even believed for a time that Markusovszky, his most loyal friend and most influential supporter, had gone over to the camp of the enemy.

ZIPFEL, 1861. In October, 1860, a serious "epidemic" of puerperal fever broke out in the Second Obstetric Clinic of Vienna. Among 101 cases of labour not fewer than 35 were fatal. The provisional Director of the School for Midwives was then the Dr. Zipfel, the person who nearly discovered the etiology of puerperal fever himself. He was by this time Professor. Professor Zipfel had to report to the Civil Authorities on the epidemic, and this report was published towards the end of 1861. "If we inquire into the etiological factors in the production of this devasting pestilence, we must observe that it broke out under the most favourable health conditions." . . . The state of the weather was perfect, according to the Report; the wards were not crowded; no case of gangrene or case producing putrid or offensive effluvia had been admitted for a long time. Postmortem examinations had been forbidden on principle the whole year through. Infection during labour by putrid or cadaveric material conveyed in the course of digital examination was not to be thought of, for every occasion to soil the hands with such substances was avoided with the most anxious care.

The first germs of pestilence must therefore be declared to be the product of some unknown epidemic influence . . . in its later developments it bore distinctly the stamp of an endemic. . . . There can be no doubt of the existence here of a miasma, or rather of a contagium generating itself within the limits of the institution. . . . Other lying-in institutions were spared, and no trace of puerperal fever in private practice at the same time could be discovered.

In order to eradicate the disease from the institution, the Second Obstetric Clinic, every means of disinfection was employed in vain . . . the institution must be closed. This last measure is the only radical cure, it is the best of antimiasmatics. . . . It is best to arrange for the patients to be confined at their own homes, and brought into the hospital 24 or 48 hours after, according to the analogy of the *Gassengeburten* which, as is well-known, are very seldom followed by puerperal illness. This from the same Zipfel who, for the sake of an immediate argumentative advantage, declared in 1850 that street-births were as often followed by fatal illness as any.

Carl Braun. There was a good deal more to record about the morbidity and mortality of childbed fever in the Vienna Lying in Hospital in 1861, besides what was contained in the Report of Professor Zipfel. Between the results in Buda-Pesth and Vienna in this year there was a dramatic contrast.

As we have seen, the University Lying-in Hospital of Pesth had been removed to new quarters in 1860, better but still very defective quarters. In the school-year 1860–61 Semmelweis did not have a single case of puerperal fever to record. In Carl Braun's Clinic in Vienna, on the other hand, a frightful pseudo-epidemic raged in the autumn; within forty-five days 113 patients sickened and 48 of them died.

It was a great scandal; to every member of the General Hospital staff the *Ætiologie* of Semmelweis was now available reading, and there was much animated discussion about the Clinic. The Director of the Clinic, Professor Carl Braun, was requested by the Directors of the General Hospital to report on the epidemic, and the measures adopted by him in dealing with it. Carl Braun reported that the cases occurred simultaneously in all the five lying-in rooms at a time when the health of the hospital as a whole was satisfactory. No rational cause could be discovered within the Clinic. On careful inquiry it was ascertained that in the end of October one-tenth of the patients admitted were in a high state of fever (heftig fieberte), and so they brought their malady with them into the hospital.

The measures adopted to prevent dissemination were: All students were forbidden to make vaginal examinations after the 1st of November.

Operation courses on the cadaver ceased for a short time. Solution of chlorinated lime was again used for washing, although it had been declared by authorities in chemistry to be useless for the destruction of organic material, and had been proved to be of no practical use in Vienna in 1854–55, and at other universities. . . .

In order to remove from the hands the cadaveric odour, permanganate of potash solution must be employed. In spite of all these extraordinary precautions 48 patients out of 253 admitted sickened in the first half of November. . . .

Referring to this outbreak of puerperal fever the editor of one of the Vienna medical journals, with the object of putting a stop to the gossip so injurious to the worthy Director of the First Clinic, published an article in which he referred to the sad illusions entertained by Professor Semmelweis of Buda-Pesth regarding the infallibility of his preventatives!

Virchow. Virchow began his investigations into the anatomical lesions produced by puerperal fever in 1846 and 1847. It should be remembered that Rokitansky was long before Virchow in these observations, and that Semmelweis, as a favoured pupil of Rokitansky, worked at the pathological anatomy of gynæcology and puerperal fever steadfastly for five years at least from 1844 until his departure from Vienna.

Virchow, recognising the infectious nature of the disease which produced the anatomical changes, called it an *ischorrhæmia*.

Not satisfied with the official report of his remarks at Speyer in 1861, Virchow published an article in his *Archiv* entitled " Investigations on Diffuse Puerperal Metritis and Parametritis." He describes simple metritis and parametritis, and proceeds with his account of the serious cases resulting from epidemic influences which present the characters of diffuse phlegmon. Among its phenomena are phlegmasia dolens. Here the lymphatics play such an important part that Cruveilhier designated the disease a lymphangitis. Without going into irrelevant details it may be sufficient to state here that Virchow speaks of necrosis as characterising the severer cases of phlegmon, and introduces his term "diphtheritic degeneration" which might affect the deeper structures or remain on the surface. He finds in the transformation of the peri-uterine tissues, including the broad ligaments and ovaries, a resemblance to a pseudo-erysipelatous condition, hence his term "*erysipelas malignum internum*." The introduction of the word erysipelas produced a certain confusion of thought among obstetricians all over the world for many years to come, and long before bacteriology began to shed its light upon the pathology of certain forms of puerperal fever.

In 1864 Virchow produced a further communication in the Gynæcological Society of Berlin on the " Nosology and Etiology of Puerperal Fever," that is to say, between three and four years after the publication of the *Ætiologie* of Semmelweis. According to the mature conclusions of Virchow the pathological anatomy of the disease must be divided into two groups—(1) diphtheritic, affecting the surface of the vagina and the uterus, and (2) the phlegmonous, affecting the deeper parts. Not satisfied with observations as pathological anatomist, Virchow goes on to explain the causes as a clinician. The internal surface of the uterus, as had been often said for a hundred years before, is to be compared to an amputation wound, and this idea introduces questions with regard to phlebitis and thrombosis. Sometimes there is a morbid condition previous to labour, but the diseases of the pathological puerperium do not appear to be metastatic. Virchow accepts auto-infection, and cannot dispute infection from without. What he has to say about treatment is singularly advanced theoretically, but his suggestions have been long forgotten by his own countrymen. In other countries with the exception of France, it has taken nearly forty years for the recognition of his principle of cleansing the "diphtheritic" wound as soon as possible after the onset of symptoms. As to the causes, epidemic origin does not cover all cases. We should recognise the merit of Semmelweis in restraining the ravages of this cruel malady, *but the infection is not such a special kind as Semmelweis alleged.* So Virchow also must be accused of writing about the Semmelweis Doctrine with only second-hand knowledge. Still this contribution by Virchow to the pathology of puerperal fever, recognising infection by some external poison, carried great influence into the practice of the subject, and although we have largely changed the nomenclature we have not even now added largely to the sum of our knowledge.

This contribution has been summarised here to show how far Virchow went in making amends to Semmelweis for what had caused him great pain. But the reparation came too late to cheer the unhappy author of the *Ætiologie.*

HECKER. As an instance of ignorant resistance of evidence we may select for this same year 1861 Hecker of

Munich. According to his report he had a "morbidity" among the paying patients of 4·9 per cent. and in the clinical division of 16·3 per cent. For this difference he could not see how the Semmelweis Doctrine could apply; he admits that the strictest cleanliness has been of advantage in surgery as well as in midwifery; but it is little use in preventing the colossal outbreaks of puerperal fever. The Doctrine of Semmelweis is " one-sided, narrow and erroneous." One more professor of midwifery who is content to raise objections in an offensive manner to the *Lehre,* which he has not taken the trouble to read, mark, learn and understand.

Hecker's blundering and ill-conditioned criticism was of service to the *Lehre* in eliciting a response from an unexpected quarter. This was a contribution to the spread of the Doctrine from Semmelweis himself, which appears to have been overlooked by his biographers. It was in the form of a letter addressed to the Editor of the *Medical Times and Gazette,* and published in that journal on June 7, 1862. It was chiefly a reply to the remarks and objections of Hecker, and it appears to have been written at the time when the " Open Letters " were receiving attention from the author and the recipients. This letter of Semmelweis is a calm and clear re-statement of his teaching regarding puerperal fever, and as such it contains nothing new. With regard to Hecker, he says Dr. Hecker makes two objections to the doctrine: (1) that it does not explain how it was that the patients fell ill in rows or sets, and (2) that it does not explain how it was that new-born infants died of puerperal fever. Hecker's objections only prove, as did everything he said and wrote about Semmelweis, that he had not taken the trouble to read the *Ætiologie*: he must have depended upon mere hearsay, that is, professional gossip.

We have seen in the first portion of the work how the " reihenweise Erkrankangen " puzzled Semmelweis at first, and how the discovery cleared away all doubts and difficulties. With regard to the " sepsis of the blood in newborn infants,'' we have read Dr. Bednar's account of its almost complete disappearance and his cordial recognition of the reason: "To the happy discovery of Dr. Semmelweis . . . who has successfully discovered the cause and the means of preventing puerperal fever.''

Yet we shall find that in the great discussion of the London Obstetrical Society in 1875, very few English teachers of midwifery appeared to know so much of the Semmelweis discovery as was contained even in this letter published in a widely-read English medical journal a dozen years before. It is also obvious that Hecker in 1861 did not know of Dr. Bednar's monograph on such an important subject as the disappearance of the fatal malady of blood-sepsis of the new-born published in 1852.

SPEYER MEETING. At the meeting of the German Society for the Advancement of Natural and Medical Science in September, 1861, the *Ætiologie* of Semmelweis was much discussed. Virchow and Hecker were decidedly opposed to the Semmelweis *Lehre;* the only cordial and decided supporter was Professor Lange of Heidelberg. Lange declared that, since he began his professorial work at Heidelberg, he had been fighting puerperal infection according to the principles of Semmelweis by the strictest attention to cleanliness and by chlorine disinfection. At a stroke the puerperal fever epidemics vanished, and although there had been a few slight cases there had been only one death from childbed fever among 300 cases of labour (v. Waldheim, p. 186).

Laying the blame of the continued devastation wrought by puerperal fever on the professors of midwifery, Semmelweis says in one of his "Open Letters": "Of the great number of professors of midwifery, within the last fifteen years only two have recognised the truth of my discovery and applied it successfully and were at the same time so honourable as to publicly acknowledge their indebtedness to me.

One of these was Michaelis of Kiel, and the other is Geh Hofrath Professor Dr. Lange of Heidelberg."

DENHAM. To this period belongs the article by Dr. Denham,[1] Master of the Rotunda Hospital of Dublin; it is the only British contribution worthy of mention, because of the writer's official position; but it is not one of which British Obstetrics need be specially proud. Denham had recently visited Paris, Munich and Vienna, and he appears to have been rather flattered by the attention which he had received from Carl Braun. He would, of course, imbibe from Braun all the anti-Semmelweis prejudices, and Denham readily and credulously received them without criticism, as did in fact with few exceptions similar visitors from the United Kingdom to Vienna for many years afterwards.

Denham began his article with a destructive criticism of all that was most advanced and scientifically true in British teaching concerning puerperal fever at that time.

After a short summary of the Semmelweis Doctrine, Denham turns his attention to the opinions of Copland.[2] He had declared that no practitioner ought to be any longer ignorant of the completely established doctrine that this most deadly of our domestic pestilences is conveyed from the infected to the healthy, chiefly and most frequently by the accoucheur.

Dr. Denham then calls to task Professor Simpson of Edinburgh who "holds strongly the infectious character of the disease, and, I regret to add, makes the doctor bear the sin and disgrace of spreading the disease to a large extent." From this disconcerting doctrine, which was just that of Semmelweis, Denham turns for comfort to Dr. Meigs, of Philadelphia, the most notorious obscurantist of his generation, and quotes him with obvious sympathy. Meigs had declared, "Still, I certainly never was the medium of its transmission." No conviction of sin and disgrace about Meigs!

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[1] On the recent Epidemic of Puerperal Fever in Dublin. *Dublin Quarterly Journal of Med. Science,* Nov., 1862.

[2] Dictionary of Practical Medicine, 1844—1858.

Speaking of vicissitudes with regard to puerperal fever experienced at the Dublin hospital, and the absence of puerperal fever in the previous decade recorded by Drs. Johnston and Sinclair, Denham asks in reference to this immunity for years: "Should we rather ascribe it to the absence of epidemic influences combined with the strict attention to cleanliness and ventilation that has at all times characterized the management of this institution?"

Coming next to Semmelweis: "With respect to the opinions put forth by Dr. Semmelweis of Buda-Pesth . . . I feel it would only be a waste of time to dwell upon them." . . . I may mention that I have lately visited the hospital in Vienna and that . . . Dr. Braun informed me that the theory put forward by Dr. Semmelweis had been entirely upset during the last outbreak of puerperal fever." So the credulous visitor believed that the final judgment had been pronounced.

Of the Dublin "epidemic" he says: ". . . There was an unusual amount of puerperal fever over the city and neighbourhood during the winter. . . . The professor at Munich mentioned to me a most interesting fact connected with the hospital there: it was opened . . . in 1859 with new beds, blankets and sheets and a new staff of nurses. Yet scarcely was it opened until they had a fearful outbreak of puerperal fever, which has visited them every year since." . . . We may recall the fact that Semmelweis had already explained in the *Ætiologie* the origin of puerperal fever in the new lying-in hospital at Munich under Anselm Martin who was a thoroughgoing epidemicist. . . . On asking the professor at Vienna whether he thought the disease was introduced or kept up by the students, his reply was short but expressive: "We have the students always with us, puerperal fever only sometimes." No doubt a clever epigrammatic way of making a statement, but defective inasmuch as it conveyed a departure from the notorious truth. Since Semmelweis left the hospital in March, 1849, puerperal fever had never ceased to haunt the

Lying-in Hospital of Vienna and at no time were its ravages more appalling than in the year before Denham's visit.

Denham, obviously under the Braun influence, says: "In my opinion the poison is often taken into the system perhaps for days before labour sets in"; a very comfortable belief. And then he asks: "May we not, therefore, fairly infer that puerperal fever possesses quite as much, if not more, of the epidemic as of the infectious character?"

Such was the doctrine of puerperal fever taught at the most important lying-in hospital in the United Kingdom a dozen years after Simpson recognised the truth, and had the moral courage frankly to proclaim his change of creed, and five years after Murphy's article in the *Dublin Quarterly*.

It only required the importation of a certain amount of French influence, chiefly under the auspices of Robert Barnes, almost to complete the ruin of British Science regarding the etiology of puerperal fever. The salutary counter-influence came from Simpson and the Edinburgh school which ultimately triumphed. But for it British Obstetrics would have deserved all the uncomplimentary criticism passed upon it by Hegar many years later than the Mastership of Dr. Denham of Dublin.

The Open Letters.

After the publication of the *Ætiologie* Semmelweis was busy for a time in sending copies of his book to his personal friends and to medical societies all over Europe, and with the correspondence arising out of that pleasant occupation. In the *Stimme* he had relieved his mind over the opposition to his teaching and practice, and he might now have turned with more advantage for the cause of progress to his own professorial duties, to gynæcology, and to his daily routine work; but he had become exasperated with his opponents, and he still watched for every reference in the medical press of

Europe to the influence of the *Ætiologie* on the spread of his Doctrine.

Within a year of the publication of the *Ætiologie* he could restrain himself no longer, and he burst out upon his chief antagonists in the Open Letters.

In these letters we find nothing that is new concerning puerperal fever: there is more emotion than ratiocination. They are perhaps best considered as the cry of painful disappointment, almost of despair, of the philanthropist, rather than of the scientific obstetrician.

The first was entitled: " Two Open Letters to Dr. J. Späth, Professor of Midwifery at the Joseph's Academy in Vienna, and to Hofrath Dr. F. W. Scanzoni, Professor of Midwifery at Würzburg." It is dated Buda-Pesth, 1861.

After recapitulating certain points with which we are familiar, and referring to a recent publication by Späth, in which the cause of puerperal fever was declared to be salpingitis, Semmelweis comes into close quarters with his antagonist.

" From these expressions of opinion the Herr Professor has given me the impression that his spirit has not been lighted up by the puerperal sun which arose in Vienna in the year 1847, although it shone so near to him.

" This stubborn ignoring of my Doctrine, this stubborn ruminating over errors, causes me to bestow upon you the following explanation:

" I carry with me the consciousness that since the year 1847 thousands and thousands of lying-in women and sucklings have died who would not have died if I had not remained silent, but to every error concerning puerperal fever which has been spread the necessary corrections have been partly made. About that, Herr Professor, you can persuade yourself that I do not exaggerate when I say that thousands upon thousands of lying-in women and new-born infants have lost their lives, who might have been saved, if I simply recall to your memory what occurred even in the First and Second

Divisions of the Lying-in Hospital of Vienna from the 1st of January, 1849, to the last day of December, 1858. . . . ."

Here follows a summary of the statistics of childbed fever mortality with which we are familiar. An attempt is made to get at the proportion of "unavoidable" or self-infection cases, and the conclusion is reached that in this period of ten years in the Vienna Lying-in Hospital at least 1,924 patients lost their lives from avoidable infection. In this number the "transferred" cases are not included, and no calculation is made of the number of new-born infants whose blood became infected from the mothers to a fatal extent. In this massacre you, Herr Professor, have participated. The homicide must cease, and with the object of bringing this homicide to an end, I shall keep watch, and every man who dares to spread dangerous errors regarding puerperal fever will find in me an active opponent. For me there is no other means for checking the murder than unsparingly to unmask my opponents; and no one whose heart is in the right place will blame me for making use of this means."

The Open Letter to Scanzoni was addressed to one of his earliest and most bitter antagonists, and but for the participation of Carl Braun, we might add, the most influential and unscrupulous of the opponents of the Doctrine.

"Herr Hofrath will have learned from my letter to Professor Späth that I have formed a determined resolution to put an end to the murderous practices, and to effect that object I have resolved to attack unsparingly all who dare to spread error regarding puerperal fever.

"In pursuance of that resolution I shall subject to criticism the essay of Dr. Otto von Franqué . . . "On puerperal illness in the Lying-in Institution of Würzburg . . . in 1859."

"With Dr. Otto v. Franqué I have no quarrel: I can only commiserate him as a man betrayed, who in good faith has acquired a fundamental knowledge of all your errors and delusions.

"The responsibility for the errors of your disciple rest upon you only, Herr Hofrath; I have therefore nothing to do with Dr. Otto v. Franqué; my business is with you alone. . . .

". . . In the essay referred to we read that in the Lying-in Institute of Würzburg . . . out of 99 patients 30 sickened from puerperal processes and 9 died.

"These cases of sickness and death were called an epidemic which was produced by certain undefined atmospheric influences.

"I deny that these cases of illness were due to atmospheric influences, and I maintain that these cases were produced because to the patients in one way or another some decomposed matter was conveyed from without, and that these are therefore cases of resorption-fever. . . . . . . Your ignorant division of inflammation in childbed into those which are not puerperal fever and those which are puerperal fever, has been already referred to. But I have proved in my treatise that your inflammations, which are not puerperal fever, are just as genuine puerperal fever as your hyperinosis, your pyæmia, and your blood-dissolution . . . All the forms of inflammation are produced by access of decomposed matter, they are all resorption-fever, and they can all be prevented by chlorine disinfection.

"You see, Herr Hofrath, how readily all the phenomena of childbed fever can be explained when the only true cause of childbed fever is recognized; whereas you explain the unknown by yet unknown atmospheric influences . . . The greatest service rendered by my Doctrine is that it teaches how the unhappiness wrought by the malady can be with certainty prevented: that it prescribes to the practitioner a recognized active method of prophylaxis. Your teaching, on the other hand, puts upon the practitioner the stamp of the Turk who, in fatalistic passive resignation, permits the disaster to overwhelm his lying-in patients."

There is a long discussion of certain anomalies of labour and the puerperium said to have been observed at Würzburg, and the iteration of the assurance that they could not be produced by epidemic influences or by the so-called puerperal miasma. In reply to Scanzoni's opinion that the midwives and practitioners in and around Würzburg do not carry infection, and therefore the cases which occur in their practice are the result of epidemic influences, Semmelweis says: "I admit that I do not share these opinions, I believe rather that the midwives and practitioners in Würzburg and its vicinity are just as colossally ignorant of the causes and prevention of puerperal fever as you are yourself, Herr Hofrath, and that the cases in Würzburg and round about are just cases of infection from without.

"It is obvious that the midwives and practitioners of Würzburg and neighbourhood have not learned in Pesth how puerperal fever is produced and how it can be prevented . . . Then, where have they learned it? Certainly not with you, Herr Hofrath, nor with Kiwisch . . . . name to me, Herr Hofrath, the professor of midwifery who has now for fourteen years been teaching my Doctrine, so that I may express my thanks to that individual alone.

"You see, Herr Hofrath, that I have drawn the props from your teaching. These you found in the murderous deeds which the midwives and practitioners of Würzburg committed in their ignorance.

"It is said that special attention must be drawn to the fact that the puerperal fever cases in Würzburg did not all occur in the practice of one doctor: naturally, for it is not one practitioner in Würzburg, but all who practise there, who are ignoramuses on the subject of puerperal fever prevention, and for the ignorance the professors of midwifery are to blame . . . and in this matter, Herr Hofrath, you have sent all over Germany a considerable contingent of practitioners who will, in their ignorance, engage in homicidal practices.

. . . You say that the cases of puerperal fever have not occurred as the result of infection from without, because your students have not made examinations. For so devoid of conscience as I consider you, Herr Hofrath, still you do not assert that all these labours were conducted to a conclusion without any examination whatever; probably you yourself, or your assistant, examine the women in labour to determine whether the case is normal or abnormal. It is all the more probable that during this pseudo-epidemic examinations were made . . . You have forgotten that two of your wards are reserved for gynæcological cases: in a gynæcological division there are often patients whose diseases produce a decomposed matter, and it is not necessary that students make examinations in order to produce a pseudo-epidemic; it suffices if the Herr Hofrath and his assistant make examinations in the gynæcological department and also in the labour-room . . .

. . . I believe much more that the midwives who examine parturient women in your hospital . . . and know as little as you do, Herr Hofrath, how puerperal fever can be prevented, when these midwives came into contact with patients who produce a decomposed material they convey infection to the healthy.

"My Doctrine is based among other circumstances upon my experience from May 1847 till now, the 25th of June, 1861, in three different institutions which were in former times annually ravaged with frightful pseudo-epidemics of puerperal fever . . .

"Your teaching, Herr Hofrath, is based on the dead bodies of lying-in women slaughtered through ignorance; and because I have formed the unshakable resolution to put an end to this murderous work as far as lies in my power so to do, I put to you the following questions . . . If, however, Herr Hofrath, without having discussed my Doctrine as an opponent, you go on to write . . . in support of the doctrine of epidemic puerperal fever, to teach your students the doctrine of epidemic puerperal fever, I denounce you before God and the world as a murderer, and the History of Puerperal Fever will not do you an injustice when, for the service of having been the first to oppose my life-saving *Lehre* it perpetuates your name as a medical Nero.

TWO OPEN LETTERS TO HOFRATH DR. EDUARD CARP. JAC. v. SIEBOLD, PROFESSOR OF MIDWIFERY AT GÖTTINGEN, AND TO HOFRATH DR. F. W. SCANZONI, PROFESSOR OF MIDWIFERY AT WÜRZBURG.

These letters contain nothing new concerning the Etiology of puerperal fever. But they are of interest as illustrating the controversial method employed in the effort to spread the knowledge of the *Lehre,* and they are remarkable as biography in indicating the eager longing aspirations of the writer for the triumph of his teaching as a life-saving message in the cause of humanity: they are of great interest too as a psychological study, inasmuch as they show the gradual exasperation of the once genial young Hungarian with the invincible ignorance and criminal negligence of the opponents in not accepting and applying the eternally true doctrine of conveyed infection.

v. Siebold, then professor of midwifery at Göttingen, was the author of the classical work on the History of Obstetrics, which was translated into French and continued to near the end of the XIXth century by Herrgott, of Nancy.

v. Siebold had made the acquaintance of Semmelweis during a visit to Vienna at the time when Semmelweis was assistant in the Lying-in Hispital. They became very good friends; and a few years later v. Siebold visited Buda-Pesth as the welcome guest of Semmelweis. In 1861 there appeared in a German obstetrical journal a contribution from v. Siebold on the subject of Puerperal Fever in which he attacked the Doctrine of Semmelweis. From the tone adopted by v. Siebold which was not unfriendly, a friendly remonstrance from Semmelweis might under ordinary circumstances have sufficed for the occasion. But Semmelweis was by this time becoming more than ever exasperated owing to the reception of his great work by the professors of midwifery in Germany, and he did not reply by a friendly remonstrance. Still we see in the Open Letter evidence, in the beginning at least, of an effort to remember the friendship of former years, and to spare the opponent who appears to have rather gratuitously thrust himself into the crowd of wilfully ignorant and ill-conditioned antagonists.

v. Siebold had written: Always keeping in mind that Semmelweis sees in cadaveric infection the chief cause, nay, the only cause of puerperal fever epidemics . . . though the matter has a good deal in it, and for Vienna in particular has been of great practical value, and there it ought never to be forgotten; still on the theory of cadaveric infection, for the present, judgment has been pronounced: it must be considered exaggerated and too exclusive. . . . It is going too far to maintain that this is the only cause of puerperal fever, and thus to explain its frequent occurrence and the malignant character, and the epidemic incidence and extension of the malady in lying-in institutions.

Such was the language of a German professor of midwifery and a man of letters used about his friend's work more than a year after the publication of the *Ætiologie,* and more than a dozen years after the discovery was first announced in Vienna and published in such a form that it ought at least to have impressed all teachers of midwifery in the German-speaking countries of Europe.

And how does the long-suffering friend so gratuitously assailed receive the ignorant and gratuitous attack?

" Herr Hofrath has made himself responsible for the diffusion of error regarding puerperal fever. . . . . I remember with pleasure the time which we spent together in Vienna. That was at the time when owing to my endeavours the First Obstetric Clinic had ceased to be a State-supported murder-den.

I remember with pleasure the time we spent together in Pesth. Pleasant memories are associated in my mind with you; but the groans of puerperal women dying of child-bed fever drown the voice of my heart; and my reason enjoins on me to make the truth prevail, even though my heart is thereby painfully touched.

There are many things in nature about which the medical profession was for a long time ignorant without human lives being thereby placed in danger.

The circulation of the blood went on for five thousand years before William Harvey discovered it, but no one died because of that ignorance. . . .

Professional ignorance of puerperal fever is not devoid of danger. . . .

" I protest in the strongest manner possible against the representation of my Doctrine by the expression ' cadaveric infection.' . . ."

" If you, Herr Hofrath, in spite of everything, thrust upon me a *Lehre* to the effect that all cases of puerperal fever are produced by cadaveric infection, that amounts to wilful misrepresentation of my teaching or want of the ability to understand it."

Here follows a long and laborious exposition of the theory of the conveyance of infection and the propagation of the malady, and, in spite of the mildness of the opening portion of the letter, Semmelweis becomes more and more excited and indignant, and addresses his old friend in the language to which his bitterest old enemies were accustomed.

" Herr Hofrath has read my book with so little understanding that the record of so many proceedings amounting to manslaughter has drawn no expression of aversion from you. You accept this devastation as something that cannot be prevented.

You have read my book with so little understanding that you still find something enigmatic in puerperal fever, whilst to those who have grasped the meaning of my teaching everything about puerperal fever is as clear as sunlight."

Discussing the opinions of French obstetricians as expressed in the great debate of 1858, and the proposal to shut up the lying-in hospitals, the writer says: "It is not the lying-in hospitals which must be abolished in order to keep the lying-in women in good health, but all the professors of midwifery who are epidemicists must be cashiered if the lying-in women are to be preserved in health. . . . I hold the opinion that in order to prevent the manslaughter of thousands and thousands of lying-in women and infants, the dismissal of a couple of dozen professors is not worthy of consideration.

" Herr Hofrath, I know you as a man of extremely kind disposition; I am convinced that it is not possible for you to do intentionally a thing disagreeable to any man. . . . . I entreat you, Herr Hofrath, to acquire an intimate knowledge of the truth as it is set forth in my book, so that according to your kindly disposition you may be able to find support for new opinions in the bright faces of your lying-in patients and—in an empty dead-house. . . .

" In the '*Ætiologie . . . des Kindbettfiebers*' there is no longer anything obscure; on the nature of puerperal fever clear sunlight has been shed; not a single point is now a mere hypothesis; in the future on these three points nothing requires further explanation."

After going over in the Open Letter the three sources of decomposed animal organic matter whence infection may spring, as in the *Ætiologie,* Semmelweis proceeds to sum up: "Puerperal fever is therefore not a contagious disease; puerperal fever is a disease conveyed to the healthy lying-in woman by means of a decomposed material the sources of which we have just now enumerated."

Semmelweis also proposed to Siebold that they should arrange for a meeting of German obstetricians in some German city in the month of August of September, 1861, and debate the question, for he held the confident belief that he would convert to his opinion every participator in the proceedings. It need hardly be said that no notice was ever taken of this proposal by Siebold or any teacher of midwifery in Germany.

Siebold took no notice of the "Open Letter." He had been ailing for a long time, and he died in October, 1861.

v. Waldheim explains Siebold's behaviour towards Semmelweis on the ground that owing to illness he had not the energy to study the *Ætiologie;* if it had been a clear concise statement of the *Lehre* he might have read it, but this monstrosity (*Unding*) of a book was beyond his powers to master. But v. Waldheim forgets that the "Open Letter" contained a summary of the whole work of Semmelweis, and that v. Siebold after receiving it, writes in his "Geburtshilflichen Briefe" about Semmelweis in a sarcastic way of having been scorched by the rays of the "puerperal sun." Besides v. Siebold had paid a visit to Vienna while Semmelweis was resident in the Lying-in Hospital, and in his "Geburtshilflichen Briefe" he writes about Semmelweis, the inspired assistant, in the same conventional terms of appreciation as he employs about his very ordinary chief, Professor Klein. It should also be remembered that it was v. Siebold who began by attacking the *Lehre*. It should be remembered also that v. Siebold had been the guest of Semmelweis in Buda-Pesth and it was impossible for him to have been a day in the company of Semmelweis at that time without learning all about the "Doctrine."

v. Waldheim's explanation is to say the least not satisfying as a vindication of v. Siebold.

Along with the Open Letter to v. Siebold appeared the second letter to Scanzoni. The occasion which elicited this Open Letter was the outbreak of a violent endemic of puerperal fever in the new lying-in hospital at Würzburg, which was provided with all the best modern furnishings and appliances. The letter is short and bitter in its tone. It recalls the statistics of the Würzburg lying-in hospital and more or less compares them with those of Vienna under Carl Braun: and it is sarcastic over the theory of *genius epidemicus* still clung to by Scanzoni.

"Herr Hofrath has been in the right for thirteen years because I have been silent for thirteen years; now I have broken the silence and I am in the right, and I shall so remain as long as the human female continues to bear children. To you, Herr Hofrath, nothing remains, if you would save your reputation or at least as much of it as remains to save, but to accept my *Lehre.* If you adhere to the doctrine of epidemic puerperal fever, then, with the advancement of knowledge, both pseudo-epidemics of puerperal fever and your reputation will disappear from the world." . . .

"Some benefit has accrued from these two pseudo-epidemics of puerperal fever in your new lying-in hospital, provided with the very best furnishings and appliances: they have completely disposed of the proposals of ignorant Frenchmen to erect new lying-in hospitals as the only way of preserving the lives of the patients. You have demonstrated, Herr Hofrath, that in spite of a new hospital provided with the most modern furnishings and appliances, a good deal of homicide can be perpetrated where the required talent in that way exists."

The last of the Open Letters is the "Open Letter to all the Professors of Midwifery." It is very long, in the original publication running to many pages. It begins by a restatement of the matter contained in the *Ætiologie.*

"In May 1862, it is fifteen years since I discovered the only eternally true cause of puerperal, no single case of puerperal fever excepted . . . in decomposed animal-organic matter." We have then an exposition of the pathology, a repetition of the statistics including those of Vienna, and of the United Kingdom, with a recapitulation of the incidents which formed the evidence on which the medical profession of the United Kingdom came to the conclusion that puerperal fever was a contagious disease. He then assumes the polemical tone and attacks Scanzoni and Braun once more. In spite of his fifteen years of preaching his Doctrine he can mention only Michaelis and Lange of Heidelberg among the professors of midwifery who have heartily adopted his opinions and acted upon them, and who have been honourable enough to publicly proclaim their convictions. " It shews how little the medical world has as yet been influenced by my *Lehre* . . . When an epidemic of puerperal fever does not excite indignation against the official persons responsible for its production, but on the contrary the observations made during an epidemic of puerperal fever are published for the instruction of the medical world."

"This fact is for me an urgent demand to work energetically for the diffusion of the truth in order to bring to an end as soon as possible this shocking waste of human life."

Although this letter occupied ninety-two printed pages, it remained unfinished; it ends with "continuation and conclusion to follow."

It is easy to understand the bitter disappointment and vexation of Semmelweis as he contemplated the obstruction placed in the way of his teaching, but it was certainly time to bring his method of advocacy to an end. As Bruck says: If Semmelweis had perceived that the method which he had adopted would not bring success, that his letters only embittered the controversy, and did not obtain for him any new friends, or if he had ceased from all further attacks on his enemies at the urgent representations of the friends, who marked with regret the excitement rising day by day which this unsparing polemic produced in him, it is probable that he would have been more successful.

In these years of bitterness when Semmelweis, misunderstood and neglected, carried on the fight with his opponents; when the whole medical press of Europe found scarcely a word of recognition for his services; in these years Markusovszky was the only friend who stood steadfast by his side, and when almost every one was beginning to doubt, never lost the courage in the endeavour to obtain recognition and adoption of the *Lehre* by his contemporaries. "The triumph of a good cause,'' he wrote, ''may well be delayed, especially when it has to do battle against erroneous teaching; yet its final success is inevitable.''

The '' Open Letters '' of Semmelweis have been subjected to much criticism which has often been in our opinion mistaken and unjustifiably severe.

It should be remembered that the ''Open Letter'' was not originally invented for the purpose of giving expression to smooth and amiable platitudes. When the advocate or the assailant of a cause resorted to the open letter, he had always something to say for which the ordinary conventional methods of address were inadequate, and if ever the open letter was justified it was at this stage of the Semmelweis controversy.

His *Ætiologie* had been published for over a year, and should have been known to every teacher of midwifery in Europe, but it was either ignored or subjected to the old misrepresentations. Even v. Siebold, the medical historian, professed to believe that Semmelweis attributed all puerperal fever to cadaveric infection alone.

We can readily imagine the disappointment and disgust with which the author of the *Ætiologie* read the ignorant and ill-conditioned attacks on his Doctrine. It was the heart of the philanthropist that was wrung by the contemplation of the loss of human life and the miseries that could have been so easily arrested. It was not the vanity of the scientific discoverer that was touched.

They reproach him with ''fanaticism''; we may accept the term and hold it to his eternal honour that he adopted the methods by which all religious and other creeds have established an influence upon some portion of the human race. What benefit was ever conferred upon a cause political or religious by a mugwump or by an elder in the church of the Laodiceans?

The opponents of Semmelweis gave utterance to the usual academic arguments, not to establish a doctrine with which their hearts were full, but to prove that the ancient formulæ were true, and their supporters were in the right. It was all heartless formal, academic, conventional. No wonder they were shocked at the want of polite 'form' in the utterances of Semmelweis.

It may be true, as Bruck says, that a more conciliatory method might have better advanced the cause, but that is very doubtful. The older professors and official teachers were already committed to antagonism, and the future of the doctrine lay with a younger and unprejudiced generation. The controversial method of the "Open Letters" was that of an honest, earnest, and vilely persecuted man, and it was probably more efficacious in the long run in attracting notice than a method more conventionally correct. Matter containing personalities is always read, and the contents of the "Open Letters" with the re-iterated exposition of the *Lehre,* would be known to men who had hitherto neglected even to give the most superficial attention to *Die Ætiologie*.

ST. PETERSBURG MEDICAL SOCIETY—1863.

The last incident in the long-drawn-out polemic on the Semmelweis Discovery which brought satisfaction and peace of mind to the unhappy author, was a letter from Professor Hugenberger of St. Petersburg, dated July 4, 1863, enclosing a copy of the report of the Proceedings of the Medical Society of St. Petersburg. This Society had discussed at five consecutive meetings the Etiology and Prophylaxis of Puerperal Fever, and the conclusion was upon the whole strongly in support of the Semmelweis Doctrine. In the "Collected Works of Semmelweis," the editor, v. Gyory, with commendable judgement, devoted over twenty pages to the report of this very important discussion, in which v. Arneth, the steadfast and accomplished friend of Semmelweis, took a leading part.*

v. Arneth had settled in St. Petersburg in an important official position, and his personal experience in Vienna and his advocacy of the Semmelweis Doctrine would no doubt have much influence with many of the members.

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* Gesammelte Werke, p. 512.

Hugenberger introduced the discussion by reading a paper on "Puerperal Fever in the Midwives' Institute from 1845 to 1859." The discussion which followed was as important in every way as that of the Paris Academy of Medicine in 1858.

Hugenberger proved by his statistics that the average mortality in the Midwives' Institute was 3·8 per cent., while during the same period the average mortality within the municipal boundaries of St. Petersburg was 0·7 per cent.

"If we formulate our conclusions," said Hugenberger, "regarding the most important causes of puerperal fever in the *Hebammeninstitute,* we see that the three sources of infection according to Semmelweis, are the principal; the rest of the cases are probably mere exceptions."

It became apparent in the course of the discussion that there were many warm supporters of Semmelweis among the members, the most influential of whom was v. Grünewald. Only two members were steadfast supporters of the pathology according to Kiwisch, and maintained that puerperal fever was an epidemic miasmatic malady.

v. Arneth spoke in support of the Semmelweis teaching, but he thought Semmelweis did not attach sufficient importance to intercurrent diseases such as pneumonia, erysipelas, etc. v. Arneth incidentally mentioned a curiosity in lying-in hospital management which shed a ray of light upon some opinions still prevailing among official teachers of midwifery. He told how Professor Seyfert, of Prague, let the normal puerperæ lie among the puerperal fever cases, because he feared that removal might injure the sufferers by its moral impression!

One of the results of the discussion was a resolution to issue rules for the guidance of midwives, and henceforth it became the duty of every Russian midwife to apply the Semmelweis prophylaxis in her practice.

Hugenberger sent to Semmelweis a copy of his article along with the minutes of the proceedings. In the accompanying letter he wrote: "You will see from this how many followers you have in the Far North, and how strongly the younger men support you. By that alone much is gained, for it is in their hands that the future lies."

Semmelweis contributed several articles to the *Orvosi hetilap* on the discussion in the St. Petersburg Medical Society. Bruck says the news from St. Petersburg had the happiest effect upon Semmelweis. It had a generally reposeful influence, and in discussions he adopted a milder tone than had been his wont for a long time past.

But his writing on the subject of the *Lehre* was nearly at an end. That the subject was exhausted for the present time was proved by the "Open Letters": the same controverted points were eternally raised: the same objections which he had disposed of times out of number were constantly thrust upon him as if they were fresh inspirations of his opponents. To answer them was a thankless and depressing task, and the emotional disturbance brought a weary, worn expression into his face, which caused much concern to his wife and intimate friends.

So, after the series of St. Petersburg articles in the *Medical Journal,* he gave up all thought of further defending his principles, and consigned the whole subject to the care of his most devoted of friends, Markusovszky.

But he still continued to contribute articles on gynæcological subjects with feverish activity, until the last; his contribution on Ovariotomy, remained unfinished in 1865.

SPREAD OF THE DOCTRINE AFTER THE DEATH OF SEMMELWEIS.

We shall consider for our present purpose all incidents in the controversy after the discussion at the St. Petersburg Society and the correspondence and contributions arising out of that episode as belonging to the period after Semmelweis, for if he was still living he was *defunctus* as far as controversy on obstetric subjects was concerned, or in fact with regard to sustained rational interest in any professional subject whatever.

COMMITTEE OF EXPERTS.

A remarkable incident of 1863 is well worthy of record. With a view to obtaining guidance regarding the proposed erection of a new lying-in hospital in Prague, Austrian government officials invited certain eminent medical men to answer certain questions concerning puerperal fever. Scanzoni's Commission of 1849 had not yet reported. Among the witnesses invited were Rokitansky, Skoda and Oppolzer of Vienna, Virchow of Berlin, and Lange of Heidelberg.

The first question formulated for them was: According to the present position of science regarding the contagious origin and extension of puerperal fever, is the theory established for certain, is it probable or is it possible?

Rokitansky, Skoda and Oppolzer considered the contagious theory of origin and extension established beyond question. Virchow declared a predisposition of the individual to diffuse and malignant inflammations to be the chief factor. . . . A local specific infection, a contagion occurs only at a certain height of the epidemic, and with a certain intensity of the contagion. . . .

Lange's opinion was that puerperal fever originated in infection by decomposed animal material. . . . The contagiosity of puerperal fever by means of a specific product must be denied.

Others who gave their opinions, such as Hecker, declared that puerperal fever was caused by injurious effluvia like hospital gangrene. . . .

We see then that a clear divergency of opinion still existed so late as 1863. Among the witnesses perhaps the most influential was Virchow, and within a year he was frankly to accept the Semmelweis pathology, simultaneously with Späth of Vienna.

SEMMELWEIS AS GYNÆCOLOGIST.

When owing to the persuasion of his friends, especially of Markusovszky, Semmelweis ceased to take any part in the public discussion of the puerperal fever question, he turned his attention more to gynæcology. This had always been a favourite subject with him, but owing to adverse circumstances, he had never been able to give it much practical attention. We have seen that when he became assistant in the First Obstetric Clinic at Vienna, he made full use of the opportunities granted him by Rokitansky to work at the pathological anatomy of gynæcology in the morning hours before visiting and examining the lying-in patients. Later he understood with horror that it is this practice which produced the frightful mortality in March and April, 1847.

He now busied himself with such gynæcological work as he could find, and tried to act according to the belief, which he had always maintained, that obstetrics and gynæcology are so intimately associated that they must be combined in teaching and practice. It was on this account that he was so distressed when, owing to the jealousy and intrigues of small rivals, he was deprived of his position on the staff of the St. Rochus Hospital, where he had gynæcological cases ten months every year. With the object of being able to give some clinical instruction in gynæcology he had always reserved some beds in the University Clinic for diseases of women.

Biographically it is of interest to recall the fact that Semmelweis found a refuge in gynæcology from the excitement, alternating with depression, which had now become so observable in his conversation about the Etiology of puerperal fever: but scientifically and historically a singular interest attaches to his contributions to operative gynæcology.

In his gynæcological practice he acted on the same principles of antisepsis as he had introduced into the practice of midwifery. It may be remembered that Semmelweis had assisted Chiari in Vienna with the simple operation of removing a fibroid polypus of the uterus, and the patient died with all the symptoms of puerperal fever. Chiari's explanation was that the *genius epidemicus* was sometimes so malignant as to carry off even women who were neither pregnant nor lying-in. This incident reverted to the mind of Semmelweis after his discovery of the cause of puerperal fever, and be brought it under the category of infection and blood dissolution produced by decomposed animal organic matter, conveyed to the site of the wound during operation.

He had still the energy to work at the teaching of gynæcology, and he was the first to make the study popular in Hungary. By the employment of antisepsis he was on the path to the introduction into operative gynæcology of the measures of prevention against infection with which we are now so familiar, and which we associate with the name of Lister. Semmelweis introduced the principles, and but for adverse circumstances the application of them would soon have developed into prominence.

He was convinced that pyæmia and puerperal fever were identical processes, and he endeavoured to secure the greatest cleanliness in his gynæcological operations, so as to prevent the conveyance, by hands or instruments of the germs of infection to his patients.

In the controversy with Seyfert[1] he recalls the incident of the death of a patient with polypus of the uterus, who died of pyæmia in the hands of Chiari in Vienna. Seyfert had asserted that pyæmia was never observed in the gynæcological departments of hospitals : Semmelweis retorted that the statement only proved that Seyfert was a bad observer. " If a blind man does not see colours, that does not prove the non-existence of colours . . . ."

"To keep to the illustration of uterine polypus; how often do such patients die of pyæmia before operation, how often do they die of pyæmia after excision? I have for six years conducted the gynæcological division of a hospital, and during the five years since I became professor, I have admitted all cases diagnosed as uterine polypus; I have had in private practice frequent opportunity of operating on cases of uterine polypus, . . . and I have not to regret a single death from the operation, I have not even seen a patient seriously ill, and I attribute my favourable results simply to the fact that I operate with clean hands." (p. 428.)

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[1] *Aetiologie,* p. 421.

Bruck says: "As a gynæcologist Semmelweis was an entirely *self-made man*." His position as Assistant in Vienna left him no time to devote to the gynæcological Clinic, but as we have seen he worked hard for years at the pathological anatomy of the subject, as only very few obstetricians had ever done. He had examined at the autopsy so many women, who had died from diseases peculiar to their sex, that he had perfectly acquired the educated touch. He could rely on it almost alone for diagnosis. The thoroughness of this preliminary training was such that his progress in clinical observation and even in gynæcological surgery was extremely rapid.

Singular light is thrown on the state of gynæcology in Hungary by the fact that Semmelweis was the first to perform ovariotomy in that country: this was in June, 1863.

There can be little doubt that it was Markusovszky who inspired the enterprising incident. At that time the operations of Charles Clay, of Manchester, Baker Brown and Spencer Wells, of London, and Thomas Keith, of Edinburgh, had attracted the attention of all the culture lands of Europe and America, and drew streams of medical visitors to witness them. Among these visitors was Markusovszky of Buda-Pesth, who came back with glowing accounts of what he had seen and heard. Semmelweis appears to have performed this first ovariotomy according to the method described to him by his friend, finishing a very unfavourable case with the external clamp on the pedicle, as was the method employed in those days even by some of the best operators. v. Balassa, the professor of surgery, acted as assistant, in the presence of the most of the faculty of medicine of the University, and a large company of medical practitioners (einer zahlreichen Hörerschaft). The patient died fifty-two hours after the operation, and the autopsy showed the condition of parts that was to be expected—under the circumstances.

In spite of this failure, the operation was taken up in Buda-Pesth and in Hungary generally, and the results in the hands of Balassa, and later of Tauffer and Kézmárszky and others, soon became as favourable as any in Europe. They had only a bad start.

The rest of Semmelweis's gynæcological work was of the usual kind, and scarcely calls for mention in detail. It was mostly published in the *Orviso Hetilap,* and therefore escaped the attention of European gynæcologists.

One contribution on Ovariotomy ran through eight numbers of that journal, and with illustrative cases in other numbers, it amounted to a monograph on the subject; but even then it remained unfinished. He wrote part of a manual of Gynæcology, and he appears to have made some progress with a work on Obstetrics, but the MS. could not be found after his death.

VII.

LAST ILLNESS AND DEATH.

During these last two years Semmelweis was ostensibly enjoying a life of calm domestic peace and happiness, but latterly there must have been times of terrible anxiety to his poor young wife, which she endeavoured to conceal from even intimate friends. Then, during lectures, at medical society meetings, even in the public street, where he became an object of derision, scenes occurred which must have convinced his medical friends that Semmelweis could no longer be considered a sane man. Their forbearance seems inexplicable to the modern reader. He might have been a royal lunatic, concerning whom no man dare take the initiative.

Many details about episodes and eccentricities are given by some biographers over which, at this time of day, we may decently draw the veil. After all the sad incidents were at an end, and Semmelweis was in the grave, the widow would naturally, in tearful reminiscences over the painful episodes, give many details in conversation; but it is enough for the medical world to-day to trace the pathetic story to its close, without the record of harrowing incidents productive of only painful regret.

On the 13th of July, 1865, Semmelweis, with his wife and children, had been visiting some friends in an outlying suburb of the city. He had behaved in such an extraordinary manner during a meal, and shewed such an unnatural and remarkable facial expression, that on the way home the dreadful thought suddenly flashed upon his wife: "*Mein Gott!* can he have become insane?" In her anxiety she sent next day for Markusovszky and related the incidents. He, while speaking words of reassurance and comfort, could not conceal his own anxiety. The mental disturbance of Semmelweis increased from day to day, and before the end of the month some deplorable incidents had occurred.

Some of the professors, including von Balassa, held a consultation and suggested venæsection, cold-water cure, and other futilities. He complained to his wife that something had gone wrong in his head, but to her and to the children he behaved "*wie ein gutes krankes Kind*" (like a good sick child).

After about a week of anxious observation, the medical friends came to the sad conclusion that the patient must be removed to a lunatic asylum in Vienna. The chief ground for this decision appears to have been the hope that some good might be done him by the care of the Director of the institution, Dr. Riedel, who had acquired a great reputation as an alienist.

So the sad journey was begun on the last day of July, 1865, by the company of friends and relatives, including wife and infant child. The following day Semmelweis was taken by his friend, Fedinand Hebra, under some pretext to the asylum, and detained there. One of the medical staff soon discovered an injury to a finger of the right hand, which had probably resulted from one of the last gynæcological operations, and had been overlooked. The wound had become gangrenous, and had perforated and disintegrated a joint. Cellulitis spread along the arm, and after the formation of metastases, the final aspect of the disease became that of pyo-pneumo-thorax, to which the sufferer succumbed on the 13th of August. Thus, within a fortnight of leaving his home in Buda-Pesth, Semmelweis was no more: he died from that disease to the prevention of which his whole professional life had been devoted—the disease which had carried off his friend Kolletschka, and put himself on the track of his discovery.

"So he died a victim to that other disease whose identity with puerperal fever he was the first to recognize, to the prevention of which, in midwifery, gynæcology and surgery, he 'devoted his energies as a teacher.' "—*v. Waldheim,* 224.

There are some other pathetic circumstances associated with the death of Semmelweis.

The body was removed to the dead-house of the General Hospital. Fifteen years before, Semmelweis had passed out through the portal in the Alserstrasse, heart-sick from blighted hopes and disappointed ambition, a broken man in his own belief; and he never returned until he crossed the threshold in his coffin to be in his turn subjected to the same process of post-mortem examination to which he had so zealously devoted years of his professional life in Vienna. The autopsy revealed extensive organic disease of the brain. Whether it was the cause or the consequence of the alternating periods of depression and excitement in an emotional type of man, is a question which we must be content to leave unanswered.

The remains were at first consigned to the grave in one of the burying-grounds of Vienna. It was not until the year 1891 that they were conveyed to Buda-Pesth and there found a more suitable and becoming last resting-place.

Markusovszky devoted to his departed friend an obituary notice in the *Orvosi Hetilap* worthy of the occasion. "He was one of those mortals not always happy, but he was favoured by fate, inasmuch as it was given to him to enrich science with a new idea, and thereby to confer upon humanity an immeasurably important service. And what still further enhances the service in this respect is the circumstance that his discovery was no mere stroke of chance, but the result of a living conclusion and conviction, evolved out of scientific observation and knowledge . . . Markusovszky also says in another place: "He was an upright natural man, and it was impossible for him to be otherwise. Egotism and cringing were equally foreign to his honourable soul."

The medical press of Austria and Germany took but little notice of the death of Semmelweis. Some short references appeared in a few journals, but nowhere was any attempt made to appreciate his personal character, or to appraise the value of his professional achievements and his services to Mankind. He was then merely a despised Hungarian 'crank.'

Now they claim him as a German, and even specifically as an Austrian German. The irony!

PROFESSIONAL SUCCESSORS IN BUDA-PESTH.

The appointment made in succession to Semmelweis was not creditable to the Austrian authorities. His place was difficult even nominally to fill; no one could really succeed him. Probably the best of the young graduates among his own former students were still too young to take the position. In any case Dr. Johann Diescher, who was not even a specialist in obstetrics and gynæcology, was appointed professor to occupy the vacant chair. Dr. Walla, of the St. Rochus Hospital, had never accepted the Semmelweis doctrine, and so it came to pass that the two official teachers of midwifery in Buda-Pesth were opposed or indifferent to the new methods of prophylaxis. So the mortality from puerperal fever began at once to rise, and it continued under Walla as in the time of Birly, the predecessor of Semmelweis. After the death of Walla, Dr. Fleischer, a disciple of Semmelweis, was appointed to the St. Rochus Hospital, and in his first year of office there was not a single death from puerperal fever. The malady was eradicated by faith and works.

When Professor Diescher retired in 1876, he was also succeeded by a disciple of Semmelweis, v. Kézmárszky, and he also at once banished puerperal fever from the University Lying-in Hospital. Since then there has been no arrest of progress in Buda-Pesth.

SPREAD OF THE DOCTRINE RESUMED.

SPÄTH, 1864.

Professor Späth had succeeded Professor Bartsch as Director of the School for Midwives in Vienna, and several times he brought his experiences before the

Medical Society, and he contributed summaries of obstetrical publications, and reviews of foreign obstetric literature, to certain medical journals. Intellectually Späth appears to have stood on a much higher level than Klein, and as an opponent of Semmelweis he was fair, exact, and conscientious compared with Carl Braun; but he was too timid and cautious to advocate unpopular opinions, or support principles until they became the accepted doctrines of the majority.

Späth was not a member of the staff of the Lying-in Hospital at the time when Semmelweis made known his discovery, but he appears to have been one of the first to recognise the truth of the doctrine, and he began quietly to apply the Semmelweis prophylaxis while professing to oppose the doctrine, or at least to ignore it. Before the introduction of the new prophylaxis Späth had reached a mortality of 16 per cent. in the Second Division; after the application of the Semmelweis prophylaxis the mortality suddenly fell to 0·5 per cent. This was a remarkable achievement on the part of Späth, considering the shocking neglect of all sanitary arrangements in the Clinic when he took office as Director.

In February, 1864, Späth read a long contribution to the Transactions of the Vienna Medical Society entitled: "Die Vorkommnise des Wiener Gebärhauses während der letzten 30 Jahren mit besonderer Berücksichtigung der Puerperalerkrankungen." The following is a summary of the relevant portion:—After a historical critical review, he gave an account of the proceedings of Dr. Semmelweis, assistant in the First Obstetric Clinic, in May, 1847, by which the health conditions of the patients were improved in a remarkably rapid manner, so that the mortality in the year 1848 finally stood at 1·2 per cent., a point to which it had never once descended since the directorship of Klein began in 1823. . . . The years 1852, 1854 and 1855 were distinguished by a high mortality, viz., in the First Clinic 4 per cent., 9·1 per cent. and 5·4 per cent. The last endemics occurred in both Clinics from October, 1861, to February, 1862, and the mortality amounted in the First Clinic to 7·7 per cent. and in the Second Clinic to 10 per cent. . . . Lastly, the endemic at the First Clinic in 1862–63 lasted three months, and reached a mortality of 6·2 per cent.

As to the cause of these endemics, Späth does not believe it to depend upon telluric or cosmic changes, and he finds no relationship with other epidemic disease with the exception of erysipelas; and in contrast to Carl Braun, he quite agrees with Semmelweis in denying any influence of the atmospheric temperature, or of the seasons of the year, in producing childbed fever. On the other hand, Späth attaches much importance to ventilation in the lying-in rooms. . . . The causes of puerperal fever endemics in lying-in hospitals are to be sought in the institutions themselves, and not outside. The injurious agents relevant to the phenomena of the disease most certainly develop their influence during the labour or in the first hours after its completion. . . . The most important, perhaps the only agent, in the production of puerperal fever is a putrid animal material, which may originate in the body of the parturient or puerperal woman herself within the hospital, or it may be brought from outside. . . . The collection of such products of putrefaction in an institution are the causes of the endemics of puerperal fever. . . .

The supply of pure air, a clear separation of the sick puerperæ from the healthy, and the thorough cleanliness of everything which may come into contact with the lying-in woman, can best prevent the spread of the disease. . . . What consideration then does the theory of Semmelweis deserve? This question arises spontaneously in the midst of our reflections; I shall not intentionally evade it, because I believe that the heated tempers caused by this subject of contention (*Streitfrage*) are again so cooled down that it is now possible to speak a quiet word on the subject. It was an unpardonable piece of ignorant levity for a certain Frenchman to say: "To call attention to this subject is to exhibit sheer inanity: and one hardly comprehends why members of the medical profession in Germany have taken the trouble to refute it." From my own experience, and from consideration of the facts mentioned, I am convinced of their great importance. This theory would certainly have attracted many friends as open supporters among obstetricians, if Semmelweis had not at first put forward as the only cause of puerperal fever the factor which was most obvious to him, and if he had not of late fought for his theory in a tone which no scientific man has hitherto employed. No one denies the possibility of producing a metrophlebitis or a lymphangitis during the examination of a woman in labour by fingers rendered unclean by cadaveric poison; no one can, however, look upon this factor as the only and actual cause of puerperal fever in a hospital. Everyone admits that by means of careful cleansing of the hands, which have been made unclean by cadaveric poison, infection during examination may be prevented, and I consider solution of chloride of lime or of permanganate of potash as quite suitable for this purpose; but no one can find in these things the only means available to prevent the inroads of puerperal fever.

Semmelweis has himself for a long time recognised the one-sidedness of this view, and he has in his work on "The Ætiology of Puerperal Fever" very clearly and sufficiently expounded as fundamental principles the opinions which I have presented, without any essential difference, as the fruits of my own experience. I venture also to express quite distinctly the opinion that there is no longer remaining a teacher of midwifery who is not in his own heart convinced of the truth of the doctrine of Semmelweis, even when he still expresses himself as decidedly opposed to it.

We give attention now to only one method of prevention, and I ask, who treats his cases in a manner not in accordance with the principles of Semmelweis? Why does everyone preach that the utmost cleanliness is necessary? Why does everybody wish the hands with which the examinations are to be made to be thoroughly disinfected by solution of chloride of lime or permanganate of potash? . . . Why do we attend to ventilation? Why do we see to it that bed-linen and utensils are perfectly clean? Why do we isolate infected cases?"

We see Späth has now reached the position of a thoroughgoing infectionist. It has been a rapid evolution: he had many doubts a year before. He has made good use of the *Ætiologie*; but he was in Vienna at the time when Hebra first announced the complete discovery of Semmelweis in 1847, and yet in 1864 he seriously declares that the *Lehre* was originally one-sided, but has undergone modification and development, and in the *Ætiologie* it has assumed a form which a rational observer can accept.

CARL BRAUN, 1864.

In April, 1864, Carl Braun addressed the Vienna Medical Society on the arrangements for heating and ventilation which had been recently introduced into the First Obstetric Clinic. Braun must have felt that the opinions expressed by Späth two months before were in some measure an attack upon himself, and a censure of the unfairness of his methods of controversy over the Semmelweis doctrine.

Even now, over sixteen years after the first article by Hebra had appeared in the Vienna Medical Journal, and over three years after the "*Ætiologie* . . . ." of Semmelweis had been published to the whole professional world, Carl Braun could not find in his heart the fairness or the generosity to make the slightest concession, openly and publicly, to the theory of puerperal infection, in honour of the man whom he had so persecuted and reviled. As Späth had just publicly declared, all other teachers of midwifery and directors of lying-in hospitals were practically applying the Semmelweis prophylaxis, whatever opinions they may have openly expressed; but Carl Braun held out.

In this address to the Medical Society, he proceeds laboriously to prove that health in the lying-in hospital depends chiefly upon ventilation! "Pure warm air in sufficient quantity is the most necessary requirement in a lying-in hospital, in order to keep down the number of cases of puerperal fever, to improve the course of the individual cases as they occur, and to diminish the mortality. Ventilation must therefore be considered the cardinal measure of prophylaxis in a lying-in hospital."

The smallness of the mortality is the best criterion of the healthy condition of such an institution, hence we are called upon to admire the effects of the heating and ventilation apparatus, as evidenced by a mortality of 2·9 per cent., 1·9 per cent. and 2·2 per cent. in the years 1857 to 1860!

No use was made of the chlorine disinfection as prophylaxis, so what Carl Braun calls the favourable health conditions (die günstigen Sanitätsverhältnisse) of 2 to 3 per cent. mortality are not to be attributed to anything that Semmelweis ever taught. According to Arneth they considered a mortality of 3 per cent. at the Paris *Maternité* something fairly satisfactory, and we have seen that Skoda referred to 1 per cent. as "the usual amount," and an object to be aimed at.

Carl Braun says, further, that from 250 to 300 students yearly received practical clinical instruction in midwifery, and half that number practised obstetrical operations upon the cadaver simultaneously. Thus "practical midwifery," that is to say, the handling of dead bodies, and clinical midwifery, went on simultaneously to a very large extent, under rather favourable health conditions; only in the winter months the results were less favourable than in the summer time.

We have seen what occurred in both Divisions of the Vienna Hospital in 1861, and shall partly repeat the account as an introduction to the later history, and to bring out how little was learned in that institution by experience. There is an English proverb to the effect that experience teaches fools, but surely the most phenomenal fools are those who are not taught by experience.

As has been mentioned in October, 1861, puerperal fever broke out suddenly, in the "epidemic" form, in both Divisions of the Hospital, even when the average mortality of the First Division was 7·7 per cent. and in the School for Midwives 6·7 per cent. In the month of November, certain extraordinary measures were adopted within the First Clinic by orders from the Hospital Department of the Imperial-Royal State-Ministry. Carl Braun was in fact compelled to introduce the Semmelweis methods of prophylaxis. No student was permitted to examine a lying-in patient during the whole month of November; none but specially selected midwives must attend the women in labour; courses of obstetric operations on the cadaver were suspended; the method of isolation was introduced, and many women attacked with the malady were transferred to other parts of the General Hospital; practical instruction in midwifery was suspended; assistants and midwives were required to use disinfectants for their hands. For disinfection of the hands a strong solution of permanganate of potash was employed, and nail brushes were introduced for the first time.

In spite of all these precautions . . . the mortality in November, 1861, amounted to 10 per cent. in Braun's Clinic, and in December to 13 per cent. . . . So chemical disinfection was declared as proved to be useless. It gave no protection against puerperal processes. Yet without closing the Hospital and without turning away a single applicant for admission, the high mortality ceased in February, 1862. . . .

In the winter months (*Beheizungsmonaten*) of the three years 1861–63 the mortality was 4·1 per cent., although besides soap and water, chemicals were employed systematically and diligently (*systematisch und fleissig gebraucht wurden*) and the results became worse instead of better.

Carl Braun next goes on to demonstrate that the prophylaxis of puerperal fever was all a question of fresh air in the wards, and of drying and warming the bedclothes and diapers. The "Aborte"—the non-water-closets—were isolated from the kitchen, and ventilated.

. . . "I believe that I must ascribe to these arrangements the greatest influence in producing the very good sanitary condition of the Obstetric Clinic at the present time."

Puerperal fever from of old until the advent of Rokitansky was considered by most physicians a disease of miasmatic origin, but the nature of the miasma had not been exactly demonstrated (*nicht exakt erwiesen*); only this much was correctly settled, that the carrier of the miasma is usually the atmosphere, and that the disposition to infection lasts only during the first eight days of the puerperium." The advent of *Rokitansky!*

There next follows a long quotation from Litzmann's work, *Das Kindbettfieber,* 1844, in support of the views to which Carl Braun is determined to cling. Litzmann's prescriptions for the prevention of puerperal fever are merely a parody of the English practice for two generations before his time. "If puerperal fever has once appeared in an institution the accoucheur . . . must never go direct in the same clothing from the room containing the sick puerperæ to the normal cases, and he must practice frequent washing with chlorinated water in order to destroy the contagion adhering to him."

"*To Professor Litzmann of Kiel belongs the priority in recommending chlorine disinfection in lying-in hospitals.*" After this singular display of ignorance and prejudice Carl Braun next goes on to say: "For ten years I have devoted the fullest attention to this subject, and I have come to the conclusion that occupation upon the cadaver through cadaveric infection cannot in any way be blamed (*durchaus nicht beschuldigt*) as a specially important cause of so-called puerperal fever epidemics in lying-in hospitals (an opinion which Virchow unreservedly now maintains), but much more must be considered the special cause of the production and spread of puerperal fever the decomposition of puerperal excreta and the suspension of effluvia in the atmosphere. . . ."

We next have some reference to recent valuable contributions to the questions of miasma, and the atmosphere in relation to disease, especially to Pasteur and the producers of fermentation, and to Dr. Mayrhofer, Braun's own assistant, who had recently discovered vibriones suspended in the air of a room. As was inevitable, Braun's interpretation of the new discoveries was deeply coloured by his own preconceptions, and was very wide of the mark. The reference to the work of Mayrhofer is the first mention on an important occasion of the beginnings of bacteriology in the investigation of the causes and prevention of puerperal disease. The work of Mayrhofer is at once commandeered to support Braun's atmospheric and ventilation theories . . . the possibility is not to be disputed that the vibriones from the excreta of healthy or diseased individuals may invade the atmosphere of a room, remain suspended, and ultimately reach the mucosa of the lungs or of the uterus, penetrate blood-vessels and give rise to fatal diseases. This brings him back to ventilation!

There is seldom in any of Carl Braun's contributions to the subject of puerperal fever an absence of some attack on Semmelweis more or less open or implicit. Here we have the usual attempt to belittle the man whom he has so long and cruelly reviled. Carl Braun and his hearers must have been all acquainted with the "*Ætiologie . . .*" published three years previously, and he no doubt smarted under the criticism and sarcasms contained in many pages of the *Opinions*. Carl Braun had discoursed on the efficacy of ventilation and cleanliness, but the advantages of ventilation and cleanliness had formed the subject of the enthusiastic admiration of Arneth, whose book was published in Vienna in 1853. Arneth also became acquainted with chlorine disinfection which had been in use at the Dublin Rotunda at least from early in the century. Yet Braun ascribes priority to Litzmann of Kiel, who wrote at the earliest in 1844.

Considering the terms in which Carl Braun describes those futile attempts to prevent puerperal fever by "systematic and diligent use" of chemical disinfectants and the practical conclusions which he draws, it may not be altogether irrelevant to mention some of the practices in the First Clinic under Braun as observed by a young Scottish graduate more than ten years after the delivery of this address, when it might be assumed that still further progress had been made towards perfect prophylaxis.

At that time many young foreign graduates attended the practice at the Vienna General Hospital, attracted mainly by the *amount* of clinical material, and it was almost the universal custom among them to take as many special courses as there was time for in a working day.

Among the courses attended by the Scotsman referred to was one of operative midwifery upon the cadaver. He obtained a card for the class on paying the number of gulden demanded, and was asked no questions as to his course of clinical midwifery or anything else.

The course of operative midwifery upon the cadaver was conducted by one of Carl Braun's assistants, a charming young fellow who did his best for the small polyglot group which formed the class. They met in a sort of dead-house in the basement, a room belonging to the department of pathological anatomy.

The subjects for operation were the bodies of women who had died in the Hospital, most likely in the lying-in division, because recent parturition would improve them as "material." By way of preparation for an obstetric operation the abdomen was opened, and the pelvic viscera were removed so as to make room for a fœtus. After the fœtal corpse had been placed in the position suitable for the operation to follow, it was held by the teacher, and the pupil proceeded to perform the operation of version, decapitation, etc., as required. Into the midst of these proceedings would come a hurried message to the effect that the assistant was wanted in the labour room upstairs to attend to some abnormal case or to operate.

There was immediately a rush of all concerned to get at the means of ablution—only a stream of cold water from a tap into a basin below, such as you can see in the scullery of the meanest cottage house in England—water cold and clear and copious, evidently direct from the Schneeberg, without sophistication by heat or chemical substances.

After this hurried and perfunctory hand-washing, the members of the class rushed off after the assistant up to the *Hof* where the *Gebärhaus* was situated. Arrived at the entrance of the labour room, each man in succession dipped his hands or fingers into a weak solution of permanganate of potash, contained in a vessel placed in a sort of vestibule. The vessel was never moved from its place, and the fluid contained in it, which looked like claret and water, served for all comers. Of real washing and disinfection of the hands there was not even a pretence. Such was the "systematic and diligent" use of chemical disinfectants which had been proclaimed as a failure in 1864; while heating and ventilation of the wards, not without plenty of warm dry diapers, gave satisfactory results.

The members of the class of operators on the cadaver were offered opportunities of giving manual aid to the assistant fresh from the dead-house in the basement. The Scottish member of the class never once made an examination under those circumstances. He had become accustomed to genuine antiseptic methods, as recently applied by Lister to surgery, with the thorough washing of the hands followed by the use of carbolic acid solution with much care and circumstance, and he found the "*systematisch und fleissig*" prophylaxis of the First Obstetric Clinic of Vienna under the directorship of the *Freiherr von Fernwald* simply revolting.

Is it not reasonable to suppose that the alleged failure of the Semmelweis method of prophylaxis at other lying-in hospitals as well was due to the same cause? The Directors were epidemicists, scoffers openly at the "doctrine," impervious to new knowledge, not severe towards breaches of the rules concerning disinfection by students and attendants. Such men were, for example, Scanzoni at Prague and Hecker at Munich. On the other hand, we see illustrated at the Vienna School for

Midwives under Späth the wonderful success achieved by earnestness and conviction with correspondingly consistent action.

Hirsch, 1864. In 1864, too late for Semmelweis to derive any satisfaction, appeared the strongest independent testimony in the whole series of publications, up to that time, while Semmelweis was still living. It was contained in the second volume of the work of Hirsch, Professor of Medicine in the University of Berlin, and was entitled: *Handbuch der historisch-geographischer Pathologie.* The second edition of this book was translated into English by Dr. Charles Creighton, and published by the New Sydenham Society in 1885. It is the 2nd Edition that is drawn upon here.

At the outset Hirsch refers to the success of antiseptics in preventing traumatic erysipelas. "The prophylactic rules, directed against hospital gangrene and septic wound-diseases, have proved not less beneficial; and for the puerperal infective diseases whose admission into the group of traumatic diseases hardly anyone nowadays will object to, the same holds good."

"Setting out from this principle (rational prophylaxis based on the causes of a disease), and working on the basis of experiences at the Lying-in Hospital of Vienna in 1847, Semmelweis developed his doctrine of the prophylaxis of puerperal fever. And although his view of the causative conditions was one-sided, he was still a true pioneer in elaborating the doctrine of the origin and prevention of that disease, and his work was of great service not only to the Vienna Lying-in Institution, but to mankind at large." I take credit to myself for having . . . stood forward as his exponent, and for having directed the attention of the profession in Germany to his measures, which had been little noticed up to that time. My endeavour was to enlarge the doctrine of Semmelweis. . . . I shall thus share with Semmelweis the credit of being named a founder of the rational doctrine of the origin of puerperal fever. . . .

Hirsch then proceeds to discuss "Puerperal Fever in former times." "There is no doubt that puerperal fever has been prevalent in all ages, just as its geographical distribution extends over the whole habitable globe. . . ." The whole subject is analysed and discussed with remarkable patience and thoroughness. Hirsch enumerated the historical epidemics in Europe from the first at the Hôtel Dieu of Paris in 1664 to the last at the Charité of Berlin in 1879.

On the question, the prevalence of epidemics according to the season of the year, he says: "Almost all the observers, both old and new . . . are agreed in saying that the epidemic or endemic prevalence of the disease falls in winter or spring; and these observations of individual authorities are fully borne out by statistics." The influence of cold weather is an indirect one. It is a change in the hygienic conditions of the lying-in hospitals, brought about by the cold season, which furnishes the real grounds for the rise of the sick-rate and death-rate.

Nothing in the history of puerperal fever, so far as we can follow it during the last three centuries, comes out so prominently and uniformly among the various factors in the etiology, as its great prevalence in lying-in hospitals, contrasting with its comparatively rare occurrence outside these institutions. For statistics on this point, in illustration of the evidence that had to be resisted by many authorities on midwifery, it may suffice to quote only one or two sets of figures. Le Fort calculated that of 934,781 women who were confined at their homes in various towns of Europe 4,405 died in childbed, that amounts to 0·47 per cent.; whereas among 888,314 women confined in lying-in hospitals, the deaths were 30,549 or 3·4 per cent. In the six great lying-in institutions of Paris the mortality among puerperæ, according to observations extending over 60 years, averaged 4·8 per cent., whereas among women confined at their homes in the city it was reckoned at not quite 0·6 per cent. Lastly, it is a noteworthy fact that of the 288 epidemics enumerated by Hirsch, 178 occurred exclusively in lying-in hospitals.

On the subject of overcrowding as a factor in the etiology of puerperal fever, Hirsch has much evidence to produce from Europe and America. He very judiciously concludes: "In estimating the importance of this etiological factor in the production of childbed fever, just as for the genesis of erysipelas, we shall have to see in these unsanitary conditions only a peculiarly favourable soil for the proper cause of the disease to develop in, or for the disease to spread in.

It is in this fact of puerperal fever seeming to break out not unfrequently quite apart from such external influences, that has given strong support to the theory of its *miasmatic* or *contagious-miasmatic* origin. It has been assumed that there is in childbed fever a specific morbid poison. This doctrine has been accepted by prominent gynæcologists of the most recent period. After an exacting criticism of all the facts which support the theory of miasma, Hirsch comes to the conclusion that this theory is completely untenable.

He then comes to the evidence of the transmission of infection. This is the conception that puerperal fever is a septic or infective traumatic malady. This theory was long ago indicated by Willis. In enumerating the causes of *febris puerperalium putrida* he says: "*Huc faciunt partus laboriosus circa uterum unitas soluta, contusio, rerum praeternaturalium retentio, dispositio ulcerosa et pleraque alia accidentia, quae necessitate quadam inducuntur.*" This theory was afterwards, *longo intervallo*, developed by Eisenmann (1837) and by Helm. The author next proceeds to give further evidence, drawn with amazing knowledge largely from British sources, and it must be said with regret that he confuses completely *Contagiostät* with *Uebertragbarkeit*, the theory of contagion with that of conveyed infection by contact with the "decomposed animal organic matter" of Semmelweis.

This question must be fully considered in the sequel under the heading of the Forerunners of Semmelweis. It is worthy of note, amidst the vast amount of evidence founded on observations which could easily have been trebled, that Hirsch mentions the name of O. Wendell Holmes as supplying only two facts to the amount of his evidence. This is at least curious considering some things which have been written about Oliver Wendell Holmes in recent years.

Hirsch goes on to say: " The significance which they (the observations) seem to me unquestionably to possess is in proving experimentally the origin of puerperal fever by direct conveyance of a noxious substance . . . . without any need for assuming there had been influences of a general kind at work in the pathogenesis. It is the great merit of Semmelweis to have solved the problem in this sense by exact research." After summarising the history of puerperal fever in the Vienna Hospital, the author proceeds to say that in this way Semmelweis founded the doctrine of the septic nature of puerperal fever. At the same time he laid emphasis on the local character of the infection, by proving that the infective matter was conveyed by the hand of the practitioner or midwife; and thus he provided a basis for the doctrine that childbed fever is a *traumatic* septic process to which every puerpera is liable. . . . The conclusion which Semmelweis drew from these facts was no doubt one-sided, inasmuch as he traced the sepsis exclusively to transmission of the so-called cadaveric poison.

On this admirably impartial and almost complete work of Hirsch two points require to be noted, one important the other comparatively trifling. These must, however, seriously detract from the value of Hirsch's work for the inquiring and critical reader. One point is that Hirsch appears to have entirely missed the difference between the gropings of the contagionists and the clear and definite doctrine of Semmelweis.

The second point against Hirsch is serious. He repeatedly refers to the doctrine of Semmelweis as "one-sided." Hirsch only knew of cadaveric poison as the alleged cause of puerperal fever, according to Semmelweis, and he actually believed that he himself discovered that the infecting matter might come from putrefactive changes essentially belonging to the putrefactive process itself. " Moreover, the infecting substance may come from the pus and ichor generated in various forms of disease. Again, experience teaches us that infection may take place by means of the patient's clothes, or through instruments, sponges, bed-linen, bedding and the like; and, to make the pile complete, by the air entering with the finger or instrument introduced into the vagina or uterus, perhaps even by means of air entering by aspiration."

There is not an idea in all this that was not expressed repeatedly, and insisted on many years before even in the first publication of the discovery by Hebra, and all the error so injurious to Semmelweis is retained a quarter of a century after the appearance of the *Ætiologie*. The *drei Quellen* of decomposed animal matter recurs many a score of times in the *Ætiologie* and the *Breife* and everywhere from the first publication of Hebra's article in 1847.

In spite of the vast service rendered to midwifery in Germany, and later throughout the civilized world, by the work of Hirsch, we are driven to the painful conclusion that Hirsch did not know the writings of Semmelweis at first-hand.

E. MARTIN, 1860–66. The Doctrine of Semmelweis met with a different reception from three different types in the medical profession in Germany. To the first belonged his personal friends in Vienna, who saw the discovery growing. Of such were Skoda, Rokitansky and Hebra, who " did not doubt for a moment " its justice and truth. Of the second class were the bitter irreconcileable antagonists, like Scanzoni and Carl Braun, with their juniors in some measure dependent upon them. To this class belonged Zipfel, Lumpe, Seyfert, Silberschmidt, Breisky. These resisted the spirit: no evidence could reach their intelligence over the barrier of mistaken loyalty, preconception and jealousy. But there was a third class of fair-minded scientific obstetricians who accepted the evidence upon due consideration when it reached them, and when convinced gave practical effect to the new doctrine. We may take as a type of these Professor Edward Martin, of Berlin.

Before the publication of the *Ætiologie* Martin, like other enlightened teachers of midwifery in North Germany, tried to prevent the spread of puerperal fever by simple hand-washing before examination, and by the abstention from midwifery practice for twenty-four hours at least after post-mortem examinations or dissection of any kind. We see in these measures probably the expression of the influence of Professor Brücke's letter written to Professor Schmidt of Berlin on the discovery of Semmelweis in 1848, and the experience of Dr. Everken at the Paderborn School for Midwives.

In 1864, Wegscheider published an account of an outbreak of puerperal fever in Berlin owing to the culpable practices of two midwives, and shortly afterwards Professor Martin described in detail the measures employed by himself at the Lying-in Hospital to prevent puerperal fever, or diminish to the utmost the number of cases. It is just the practice universal in England for half a century, with some improvements. The greatest care was taken to keep the person of the patient thoroughly clean, and to renew the bedding and bed-linen for each individual case: catheters and nozzles of douche-apparatus must be boiled before use. Among the measures of precaution it is interesting to find that nurses and midwives were forbidden to lay out the bodies of patients who had died in childbed. It took forty years more to introduce in England a rule to the same effect—still probably honoured more in the breach than the observance. Martin gave a fuller account than ever before of his *nostrum,* the import of diphtheritic inflammation of the female genitals, of the methods of its conveyance from the infected to the healthy, and consequently of its prevention. He is perhaps also the first to call in question the influence of the atmosphere as a carrier of infection: "Whether this contagion can also be conveyed by the atmosphere is very doubtful."

This idea we find among the prominent influential and independent obstetricians next expressed by G. Veit and Winckel in 1866. But before the publication of v. Winckel's book a great change had come over obstetric opinion in Germany. Virchow and Veit now more or less frankly admitted conversion to the Semmelweis Doctrine; in fact no prominent teachers remained in opposition except Scanzoni and Carl Braun, and their satellites.

It was in the year 1860 that Professor Martin first enunciated his theory that puerperal fever depended upon a diphtheritic process set up in the female genital organs. He returned to the subject in 1871. The English reader will find an unusually extensive summary of Martin's paper in the *Medical Times and Gazette,* November, 1871. In this paper Martin appears to be describing for the first time the pseudo-diphtheritic membrane with which we are now familiar as a phenomenon of sepsis, chiefly observed on the surfaces of lacerated parts. "In the majority of cases we find on the external genitals and the vagina a diphtheritic deposit covering those wounded spots. . . ." Bacteriological science was not then sufficiently far advanced to enable Martin to differentiate between true diphtheria and sepsis with membrane formation.

But on the whole Martin must be looked upon as an object for the commiseration of the gods, a good man struggling with adversity: he assigns to "diphtheria" too important a part in the pathology of puerperal fever, and his thesis could not be proved.

With regard to etiology, he gives some graphic details, though of course in principle there is nothing new—for example, as incidental illustration: "This is very positively shown by the well-known fact, confirmed by the numerous figures of the Vienna Lying-in Hospital, as also by the results observed in my own Clinic, that the so-called street-births (*Gassengeburten*) are scarcely ever followed by puerperal fever." It will be remembered that this was denied by Zipfel and others for a purpose —opposition to Semmelweis. Then again: "The epidemic prevalence of puerperal fever in Berlin during the winter of 1870–71 may with strong probability be attributed to the employment of so many of the civil practitioners in the military hospitals."

It is not possible from all this to evade the conclusion that Martin had accepted the Semmelweis *Lehre* in full, though it would have been somewhat irrelevant to have introduced a formal confession of faith in this particular contribution on the subject of puerperal fever.

ROBERT BARNES, 1865. By an article by Dr. Robert Barnes, published in the *Lancet* in 1865, the reader is carried away into the past, and then set aside to repose in a back-water outside the stream of progress. We think it is no exaggeration to say that the influence of Robert Barnes, in spite of the enlightened teaching of Simpson and Matthews Duncan and some provincial lecturers to counteract it, hampered the introduction of scientific principles with regard to puerperal fever in England, for a quarter of a century.

According to Barnes in 1865 pregnancy often produces a profound alteration of the blood. . . . Mauricean called pregnancy a disease of nine months. . . . Boerhaave said: Foemina plurimis afficitur malis ex solâ graviditate. . . . Before labour comes on the constitutional conditions arising out of pregnancy have already developed a high susceptibility to the invasion of febrile and infectious disorders. . . .

In 1866 appeared the work of Professor Winckel of Rostock, on the "Pathologie und Therapie des Wochenbetts," which attracted general attention among obstetricians even beyond the boundaries of Germany.

It is disappointing to find Winckel alleging that Semmelweis in 1847 declared cadaveric poison to be the chief cause of puerperal fever, and gradually evolved his complete theory as proclaimed in the *Ætiologie* which was published in 1861. Winckel speaks of the theory that Semmelweis " so fanatically preached," and yet although he was preparing a monograph on puerperal fever for the much required instruction of his fellow countrymen, he permitted himself to make use of an opprobrious epithet, while clearly unimpressed with the vast services which Semmelweis from the first rendered to obstetric science.

VEIT, 1867. Professor G. Veit in a chapter of Virchow's *Handbuch,* published in 1867, speaks frankly and without reservation of the merits of the Semmelweis *Lehre*: "The explanation of puerperal fever as a resorption fever produced by infection by means of a decomposed animal material, has in recent years been accepted by an ever-widening circle, and ere long it will meet with no opponents. . . ." The various pathological processes in puerperal fever are completely explained in the theory of septic infection. . . .

A large part of Veit's article is devoted to a criticism of the contributions to the Vienna Medical Journal in 1864 in praise of heating and ventilation as the best means of preventing puerperal fever, and of Braun's declaration that disinfectants are of no use. . . . " Only a firm conviction of the usefulness of these measures is a guarantee that the efforts necessary for disinfection will be made in all their completeness. . . . Veit gives expression to some sound practical judgment with regard to the beds of lying-in women, the boiling of catheters, and the complete disuse of sponges. Veit was by no means a zealot, but v. Waldheim is perhaps a little exacting when he blames him for the too cautious and unprogressive statement of opinion prevalent in some parts of Germany: " As to disinfection, there may be something in it."

A much more interesting reference is made by Veit to Vienna than to Carl Braun and Späth; that is to the work of Mayrhofer, who in the course of a series of observations published in 1863 to 1865 had come to the conclusion that it was the examining finger, and not the atmosphere, which was the carrier of the "vibriones."

SCANZONI, 1867. We have heard perhaps too much of Scanzoni in the controversy concerning puerperal fever. His special opinions on the cause and nature of the malady were never of any importance to the world: it was by reason of his official position and influence, as affecting the dissemination of the *Lehre,* and the personal feelings of Semmelweis himself, that Scanzoni has been of interest to unprejudiced observers of the controversy. Now when all the old opponents are more or less frankly admitting defeat and joining the stream of progress the attitude assumed by one of the most bitter and unscrupulous antagonists of Semmelweis becomes a subject rather of psychological, than of scientific interest.

In producing a new edition of his *Lehrbuch* in 1867, it was necessary for Scanzoni to define his attitude toward the Semmelweis Doctrine. He went through the painful ordeal according to the methods of controversialists of his kind. "We must look upon it as an achievement of recent times that the import (*Begriff*) of this extremely pernicious . . . malady has been now more exactly determined. . . . To the untiring research of the last ten years, we are indebted for the fact that puerperal fever is now almost unanimously considered to be an infectious disease, which . . . is essentially characterised by the symptoms either of pyæmia or of sepsis, preceded by the admission of products of putrid decomposition of animal matter into the blood-mass. . . . It is by no means a new conception that compares every newly-delivered woman as a wounded person presenting not one but a very important number of wounds. . . . The untenability of the hypothesis that puerperal fever can be spread by means of a contagium is now placed beyond all doubt; and Hirsch has illuminated the question with few but striking words. . . . Of an epidemic in the true sense of the term we can no longer speak. . . .

Instead of considering the air of a lying-in ward of a hospital to be the chief vehicle of infection "some have endeavoured to explain this observation in another way. This explanation is to the effect that the carrying of deleterious matter from one sick patient to another is not effected through the medium of the atmosphere, but that the putrid material is transferred by means of the hand of the examining practitioner, by means of insufficiently washed bed-linen . . . an opinion which in the most recent years has found a very zealous champion in Semmelweis. . . ."

Hecker has declared that this standpoint is one-sided, narrow and therefore erroneous, and Scanzoni must equally to-day, as eighteen years before, declare in the most decided way that it is sheer waste of labour to endeavour to explain the frequency and virulence of the puerperal diseases of lying-in hospitals in this way alone. . . . "We are still of the opinion that it is chiefly miasmatic influences in lying-in hospitals which lie at the root of the diseases most frequently affecting lying-in women. . . . It affords real satisfaction to observe that Semmelweis, who at first attributed puerperal infection in lying-in hospitals almost exclusively to cadaveric poison, felt compelled later to assign a suitable recognition to other ways of infection. . . . Further we cannot and will not leave unmentioned that Semmelweis, by his restless and self-sacrificing efforts in this field, has achieved a great service to lying-in women in our hospitals. . . ."

The reader who has observed with some modicum of attention the course of the controversy, since Scanzoni first attacked Semmelweis until now when the author of the *Ætiologie* was under the sod, must be shocked by the hateful and unprincipled controversial methods here introduced (*suggestio falsi, suppressio veri*). Progress only during *the last ten years*: the hypothesis of spread of puerperal fever by contagion has been rendered untenable by *Hirsch:* it was Skoda who first published the incomplete doctrine of Semmelweis: the corruption of the atmosphere is the most common cause of puerperal infection, and Braun and Späth of Vienna have obtained a magnificent success with their ventilation apparatus; Semmelweis, not Scanzoni, felt compelled to modify his opinions regarding the etiological factors of puerperal fever: Scanzoni was right after all in his opinion about the miasmatic influences at work in lying-in hispitals.

v. Waldheim very clearly and thoughtfully remarks on this last appearance of Scanzoni: "Scanzoni had perhaps succeeded in deceiving his contemporaries as he certainly deceived his assistants, his students, and the members of the Würzburg faculty; perhaps he even came to believe the lies with which he crammed other men, but he could not deceive posterity. We have all his utterances complete before us, and our verdict on his behaviour can be nothing short of crushing." When we recall the persistent attacks of Scanzoni upon Semmelweis, both direct and indirect, and the torture which he inflicted upon the genial and unselfish author of the *Ætiologie* through a long series of years, we welcome the uncompromising judgment of v. Waldheim with which we heartily simpathize. It is "la justice et la verité."

By way of contrast with the attitude of Scanzoni v. Waldheim quotes from Puschman the story of Peter Frank which we welcome as a relief. Peter Frank was a famous character of the old Medical School of Vienna. He admitted in a quite straight-forward and honest way when he had made a mistake. His son Joseph used to tell that nothing ever pleased his father better than to have to anounce to his students: "Gentlemen, strike out all these passages in my book! When I wrote them I thought they were true: now I am convinced of the very reverse."

It is mere platitude to say that few writers on medical subjects have had the moral courage to admit mistakes, although such frankness would probably be the best of their teaching influence. It is a thing to recall with pride that British Obstetrics has produced a dignified Peter Frank in Simpson of Edinburgh, and in Leishman of Glasgow.

BOEHR, 1868. So long and persistently had the old error with regard to the one-sidedness of the *Lehre* of Semmelweis been disseminated by the older teachers of midwifery, that the younger man accepted the error in good faith as the historic truth, and it became ineradicable, in Germany at least.

In May, 1868, Dr. Boehr gave an address in the Berlin Obstetrical Society on the theory of infection of puerperal fever. "My task to-day is to call your attention . . . to the *Species femina obstetrix infectrix ignorans,* and to restrain their action. . . ." Then comes as usual the constantly reiterated error: "In his work published in 1861 . . . . Semmelweis enlarged his ideas which originally attributed it to nothing except to a cadaveric principle by admitting as cause every putrid infection emanating from an organism living and diseased . . . . Then follows a very fair exposition of what the Semmelweis doctrine of the etiology really was as explained in Vienna from the publication of the first article of Hebra in 1847.

V. WINCKEL, 1869. In the second edition of his work, published in 1869, Winckel maintained the old error with regard to Semmelweis, but he did good service in spreading the true doctrine in his references to ventilation:

"Necessary as good ventilation is, it is never sufficient to permanently counteract the spread of puerperal processes in large lying-in hospitals, or to completely suppress it as Braun professes. The favourable results which have been obtained in Vienna, and ascribed to the ventilation apparatus, are certainly not owing to that cause alone. The apparatus was introduced for the first time in 1864, but according to Späth's report the mortality in both divisions of the Vienna Obstetric Clinic had sunk to 1·06 per cent. in 1863.

v. Winckel might have added that Späth frankly attributed his success to the introduction of the Semmelweis prophylaxis: it was Carl Braun who declared the value of disinfection disproved, and obviously exaggerated the efficacy of ventilation and warming.

Mc. Clintock, 1869. We have read with regret the opinions of Dr. Denham, published in the *Dublin Quarterly*, 1862: we think it interesting to read an account of Dr. Mc. Clintock's conclusions seven years later.

"On the production of puerperal fever by inoculation by the accoucheur," by Dr. Mc. Clintock, President of the Pathological Society of Dublin. *Dublin Quarterly*, August, 1869.

"Though but a limited contagionist, still I hold very strongly the producibility of puerperal fever by inoculation; as, for instance, where the hands of the accoucheur, or those retaining some necroscopic matter or septic poison of any kind, come in contact with the highly absorbing surfaces of the maternal canals. The experience of Dr. Semmelweis at the Vienna Lying-in Hospital is, no doubt, familiar to you all . . .

"I cannot help thinking that if students, while attending at lying-in hospitals, were precluded from dissecting or from being dressers at Surgical hospitals, it would help to lessen the frequency of puerperal fever. I also think it would be well if medical officers of lying-in hospitals were to abstain altogether from taking any share in post-mortem examinations. . . ."

He went on to say that he very much blamed himself for not having rigidly acted on this rule when in medical charge of the Rotunda.

Dr. Mc. Clintock could be no longer a young man in 1869, but he had not ceased to observe and learn as had done some of his contemporaries and juniors in the United Kingdom.

Schroeder, 1871. Among the ablest of the young professors of midwifery of the Franco-German War period was Karl Schroeder, then professor at Erlangen, and afterwards at Berlin. In his *Lehrbuch,* published in 1871, he wrote:

"In the most recent time the opinion supported in this book regarding the origin as depending upon the resorption of septic matter from a wound has been constantly gaining ground . . . In fact, the man who carefully peruses the works of Veit and Hirsch, and still doubts the possibility of the production of puerperal fever by the resorption of a decomposed organic matter, such a man is beyond the reach of argument . . . The first who expressed the belief that childbed-fever could sometimes be conveyed by doctors and midwives, who had to handle or deal with cases of puerperal fever, was Denman. Very soon, in England, evidence in support of the manual conveyance (Ubertragbarkeit) of the disease was piled up, and a great number of observations were contributed, which went to prove that lying-in women might be infected, not only by women suffering from puerperal fever, but from cases of phlegmonous erysipelas or filthy wounds, through the hands of the practitioner . . . More weighty in its consequences . . . was the discovery of Semmelweis who, everywhere, when there is anything said about the benefactors of the human race, deserves to be placed in the first rank. He first came forward in 1847 with the thoroughly one-sided and insufficient opinion that puerperal fever depended upon cadaveric poison, but he extended his views of his own accord so wide that we now must regard the dominating opinions concerning the etiology of puerperal fever as essentially his own, and the result of his own merit."

Spiegelberg, 1874. In 1874, Professor Otto Spiegelberg, of Breslau, gave two lectures on the essential elements of puerperal fever (Uber das Wesen des Puerperalfiebers). We shall meet with these lectures again in connection with Waldeyer's contributions to bacteriology; we must glance at them now from the clinical standpoint. "Who was the carrier: whence came the disease, why was the progress so rapid? Perhaps a miasma! That cannot be: a miasma behaves differently. Gentlemen, I can tell you: the carrier of the infection was the attendants, the bringer an accidentally infected puerpera . . . They were wont to seek the origin of the disease in the corrupted atmosphere, in want of cleanliness, and that was at first the conventional service of defending the infection theory rendered by the English and, among Germans, by the much-abused Semmelweis. In the clearest manner these causes were expounded and followed in all their consequences by Hirsch, Veit, and v. Winckel."

We have often wondered on what principles writers on puerperal fever have selected their authorities. Why should Spiegelberg have selected Hirsch, Veit, and Winckel? Able and accomplished men all of them, but what did they accomplish in improving upon the *Lehre* of Semmelweis? It required neither extending nor mending at the hands of any man. It was complete, "totus teres rotundus," as it came from its creator within the Vienna period; and although in the course of time scientific research has thrown light on the reason for the success of the prophylaxis, the experience of every decade only adds to the conviction that Semmelweis alone completed the *Lehre* on its clinical side in every phase and form of it, and gave practical proof to the whole world how life could be saved by it.

Carl Braun, 1881. In preparation for the publication of a second edition of his *Lehrbuch*, in 1881, Carl Braun was compelled to give expression to the opinions which he had come to form on the theory of infection. It was a task that could not be shirked. He had by this time occupied the chair of midwifery in Vienna for a quarter of a century: he was now the oldest living of the opponents of Semmelweis and his *Lehre,* and never had antagonism on a scientific subject been more bitter in its expression and more unfair in its methods, than that of Carl Braun during at least the first ten years of his directorship of the First Obstetric Clinic. Carl Braun could now, in 1881, have afforded to be fair and even generous to Semmelweis, but he appears to have been unable to rise to the occasion. Under the heading of "Ætiologie des Puerperalfiebers," after a short historic retrospect, he said: The conveyance (Ubertragbarkeit) of an infectious matter has been proved in a large number of cases, and the greatest cleanliness and precaution has been prescribed Semmelweis (1847), Winckel (1869), Spiegelberg (1870) . . . Lee (1875) . . .)

Pyæmia took the place of infection by cadaveric poison, and the opinions and observations of surgeons upon blood-poisoning from wounds were now accepted as the chief etiological basis of puerperal fever: thus it has come to lose everything characteristic . . .

"According to Semmelweis (1847), and Lange (1862), puerperal fever arises from a blood-disease produced by infection from a decomposed animal matter . . .

"Most gynæcologists of recent times have expressed the opinion that puerperal fever may arise spontaneously, and that it then becomes infectious to healthy women during labour or in childbed, and that the infection may then be spread by foul air penetrating inside the genitals or by the introduction of unclean hands or instruments."

Here, then, is the best that Braun could find it in his heart, even at the last, to say about Semmelweis. He cannot mention any feature of the *Lehre* for which the world was indebted to Semmelweis alone, without dragging in the name of some writer on the subject who had learned from Semmelweis, even though, like Schröder and Lange, they had gratefully and most explicitly proclaimed their sense of obligation to Semmelweis—Lange fifteen years and Schroeder seven-

and-twenty years after the first Hebra article! Why was such a thing done? Was it ignorance or stupidity? No one who ever knew Carl Braun could excuse him on such grounds. Semmelweis was quite right in setting down Braun's first attack upon him, over thirty years before, to ill-will and jealousy. Why do we rake up all this now? Because history is only worth writing or reading if it guides posterity to practical conclusions for its own use; if it guides or warns. The time is now sufficiently remote to hurt no susceptibilities, and, in leaving Vienna and the persecution of Semmelweis, we may recall once more the sentiment: *On ne doit pas aux morts que . . . la justice et la verité.*

W. A. Freund, 1885. Passing over the less important episodes, we return once more to Strassburg, now become a German university city, to call attention to the generous and emphatic appreciation of Semmelweis by Professor W. A. Freund in 1885.

Freund's subject was Parametritis, and that naturally brought him to refer to Virchow in discussing the pathological anatomy. In a note, Freund then went on to say that whoever painfully reflects on the eighteen years' delay in the recognition of the great discovery of Semmelweis, which for the sick and sound patients brought such blessings, for physicians such saving from worry and anxiety, if such a one omitted the name of Virchow from the record of obstruction he made a mistake. Virchow's authority did not stand out prominently in clinical etiological research, and there was no reason why he should be appealed to on such questions. Virchow's intervention in clinical etiological work was obstructive. While we are in the enjoyment of a benefaction conferring great happiness, we associate with the thought of the benefactor our resentment against those who, for a long time, obstructed the way to our enjoyment. We sit in judgment. Do we not know the course of history? Do we not know that the mighty achievement of Harvey required thirty years for its recognition . . . Semmelweis was a vessel too small for its contents. He was full of genius as a discoverer, highly capable in laying hold of the discovery, weak with regard to bringing it into full effect during his own time. Consistently with his endowments and according to the circumstances of the time, has he fulfilled his own personal destiny; at one time cheered and made happy by recognition of the new truth; again bitterly resentful if it did not at once receive recognition, chiding against all opposition; at last fallen into wreck and ruin. "When Fate calls upon such natures to play the part of prophets, the performance is always a tragedy. Fortunate for mankind if the prophecy is not overwhelmed with the prophet."

Obstetrical Society of London, 1875.

For more than half a century before the discovery of Semmelweis, the science and practice of Obstetrics in the United Kingdom of Great Britain and Ireland was generally recognized as the most advanced and successful in Europe. In the prevention and treatment of puerperal fever we were unquestionably the most successful, and were naturally assumed, therefore, to be the most enlightened. Foreign visitors came to London and Dublin to learn the secret of our success, with the intention of introducing our principles and methods into their own native lands. Of these the most distinguished was L. J. Boër of Vienna, commissioned by the Emperor Joseph II. It will be remembered also that Semmelweis, in the prospect of a set-back in his professional career in 1846, commenced to learn English in preparation for visiting the Rotunda Hospital of Dublin with the object of working at clinical midwifery.

From the fourth decade of the nineteenth century a process of deterioration had set in, largely under French influence. Men with special endowments, such as Simpson of Edinburgh, rose into eminence now and again, and produced a salutary influence for a time, but there was no general progressive movement, and from the time of the introduction of anæsthesia into obstetric surgery the incidence of puerperal fever in particular appeared to become more general and more marked.

The chaotic state of English opinion on puerperal fever was well shewn in the great debate in the Obstetrical Society of London in 1875. The Discussion* began at the April meeting and continued to July. and during its course the chief authorities in Obstetrics and Gynæcology in Great Britain, and some who were not authorities on anything, gave ample expression to their opinions.

The discussion was on the "Relation of Puerperal Fever to the Infective Diseases." Spencer Wells, in opening the debate, quoted from the Nomenclature of Diseases the definition of the term puerperal fever as given by a Committee of the Royal College of Physicians of London, and considered it to be the most accurate and comprehensive. The definition is this: "A continued fever communicable by contagion in connexion with childbirth, and often associated with extensive local lesions, especially of the uterine system." Spencer Wells stated, in the course of his address: "I must ask you to say if, in your experience, you ever saw such a case which could not, on careful inquiry, be traced to exposure of the patient to some one or other of the contagious or infectious fevers—to scarlet fever or diphtheria—to measles or smallpox? I need not remind you how these diseases are intensified or modified by the puerperal condition; and I proceed to ask if, in any case where puerperal fever could not be proved to be really scarlet fever, diphtheria, measles, or smallpox, occurring in connection with childbirth, it was not a traumatic or surgical fever, erysipelas, pyæmia, or septicæmia; the local lesions associated with the fever assuming rather a primary than a secondary importance in the chain of sequence."

After reference to the appearance presented by the visible parts injured during parturition, he asks: Does all this arise under perfect sanitary conditions, spontaneously, or from mere chemical decomposition, or only when some poisonous agent is introduced from without, the seed of some plant sown in a fruitful soil?

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* Trans. of the Obstetrical Society of London, 1875.

. . . What relations do this fever and the local lesions bear to one another? . . . What relation have bacteria and allied organic forms to the pyæmic process in the puerperal state?

What is the value of antiseptics in the prevention and treatment of puerperal fever?" The speaker then gave a summary of the contents of a paper of his own, published in 1864, discussing the researches of Pasteur on fermentation, on the organised corpuscles in the air, on spontaneous generation, and on putrefaction. He had also shewn from the observations of Angus Smith and others, that germs may often be found in the air of crowded rooms and hospital-wards, which only require favourable conditions for their rapid development. . . . It is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or in pus, and that they so modify it as to convert it into a poison when absorbed . . . or they may enter the blood, effecting in the process deadly changes in the circulating fluid. Reference was next made to the recent researches on bacteria by Davaine, which received too little attention in this country . . .

. . . Lister's work was begun at the Glasgow Infirmary in 1866: he used carbolic acid for the express purpose of destroying the organic germs in the air, or in any of the liquids about the patient, or, of protecting any wounded or injured part . . . the results are too well known to require more than the most passing allusion to the prevention of surgical fever, of pyæmia and septicæmia . . . and the general freedom of hospital-wards from noxious odours and matters, and from the introduction of poison from the dead-house or dissecting-room . . .

If traumatic fever and pyæmia can be kept out of a surgical hospital why should not puerperal fever be kept out of a lying-in hospital . . .? There has been a great outcry against lying-in hospitals of late; but I trust this Society may be able to guide the feeling rather in the direction of freeing them from puerperal fever than of destroying them . . .

Strict enforcement of the same laws of sanitary science, which has reduced the mortality after ovariotomy, should be "your rule and guide in your daily practice."

After the conclusion of the opening address, a letter from Dr. J. Matthews Duncan was read, in which, among other things he said: "I have always practised . . . on the footing that care of the hands and clothes is a sufficient precaution for the safety of my patients. I have no belief whatever in any special virtue in going away . . ."

Dr. Leishman, professor of midwifery in the University of Glasgow, regretted that he had disseminated views which he now believed to be erroneous. He regretted that he had not more thoroughly investigated the evidence of the pyæmic source of puerperal fever. He believed there was strong evidence in favour of pyæmic or septicæmic origin, but he was not prepared to go the length which many writers in Germany, and some in this country, had gone in accepting the pyæmic theory as the solution of all our difficulties. His own difficulties arose from the class of cases connected with or produced by a specific poison such as scarlatina; and there was another class in which the original symptoms were more those of a local inflammation, be it metritis or peritonitis, localised or general. As far as the final symptoms are concerned there is a difficulty in separating this class of case from those cases in which the puerperal fever was dependent on pyæmia or septicæmia . . . We have frequently been informed that these diseases are likely to be engendered by decomposing animal matters from whatever source the decomposition may originally arise . . . Objections were taken by the speaker to the Semmelweis doctrine as not sufficient, and the work of the late Sir James Simpson and Professor Lister were drawn upon for supporting evidence . . . "This identity of pyæmia or septicæmia with puerperal fever may be established within certain limits or it may not; but there is an intensity in the infection . . . in the woman in the puerperal state, a peculiar condition . . . which we know very little about . . .

Here we have once more the position taken up by Churchill in 1849, and by so many German professors contemporary with Semmelweis. There is something more in it than mere local wound-fever: there is "the unknown something," the *divinum aliquid,* which is a mystery.

Dr. Newman, of Northampton, a medical practitioner, made some remarks which are of interest as showing the opinions with regard to puerperal fever which were prevalent at that time among the best class of general practitioners. From the experience of twenty years he had reached the conclusions: firstly, that there was no such thing as a definite puerperal fever, and in the second place, that there was always some definite infecting poison. This "definite" poison turns out from the illustrative cases given to be scarlatina, "a local inflammatory mischief" illustrated by manual removal of the placenta, sewer gas, the mental conditions associated with pregnancy, and the peculiar constitution in the puerperal state—curious explanation of opinions concerning "definite poison." Dr. Newman thus made a disappointing contribution to the discussion.

Dr. Braxton Hicks, Lecturer on Midwifery at Guy's Hospital, considered that the solution of some of the difficulties had been retarded mainly by two influences: (1) Most of the older observations were made in hospitals, and thus the character of the cases was similar; (2) information was obtained from the post-mortem room, hence the appearances most prominent were supposed to be of the essence of the disease instead of being looked to as effect. This concentration of attention obscured the clinical facts. Investigation into the nature of the poison is a distinct part of the subject. We must not look at the death-rate only as a measure of the influence of the disease. Where one dies three or more are retarded in their recovery by secondary effects such as cellulitis, phlegmasia, etc. Quoting a paper of his own, the speaker said that out of 89 cases he had found that 68 cases had been connected with some animal poison: of the 68 more than half had been connected with scarlatina "in one way or another." Amongst the remainder erysipelas, diphtheria, and an offensive state of the discharges were prominent. Of 21 cases there was no definite history respecting zymotic disease . . . Some had undoubtedly been exposed to mental depression or excitement, or they were in a low state of health. Some 4 of the 15 left were ill before labour . . . Some were of traumatic origin, not toxæmic . . .

Now where is the proof of the existence of a separate entity such as ordinarily understood by puerperal fever? . . . The specific symptoms of zymotic disease were of all grades of proportions in the several cases; and, generally speaking, the less the specific signs shewed themselves, the more tendency there was to malignancy. . . ." Illustrations by some cases shewing credulity regarding scarlet fever—with the entire overlooking of an incubation stage. Usual argument about women in childbed recovering from scarlatina—"Even so; but this is no proof that the influence of scarlatina is not detrimental" . . . Violent mental emotions are also followed by symptoms precisely similar to those that follow zymotic influence . . . In some cases, no doubt, some other medium must be added, such as decomposing "sepsis," or the living bacteria . . . That decomposing matter does cause these symptoms can readily be proved . . . One thing seems to militate against the notion that it is the bacteria which accompany the absorption of offensive discharges which produce the symptoms, namely, that if you wash out the uterus the symptoms very rapidly subside . . . if living growths were going on, one could scarcely expect so rapid a subsidence . . . "No practical treatment can be founded on knowledge of bacteria" . . . !

Such is the liability of the puerperal women to these determinating influences that we may generally trace nearly every ill-getting-up to some depressing or disturbing influence . . .

The "intensification" of the puerperal fever is explained by the peculiar condition of the blood and the impressible nervous system . . .

. . . Respecting the contagious nature of the conditions grouped together as puerperal fever, the majority are contagious to puerperal women; whether all are so is uncertain. Those forms derived from the zymotic diseases are most contagious: those self-generated the least so. Some few seem not at all contagious . . . "But, surrounded as we all are by contagion, it is very difficult to say how far any case is free from zymotic influence."

Mr. Jonathan Hutchinson, even then a distinguished surgeon, had nothing to do with obstetrics or with puerperal fever, but he believed that the subject had analogies in general surgical practice . . .

"I express in the most unqualified terms my belief that erysipelas is not a specific fever, that it is only a local form of inflammation . . . the pyrexial symptoms and general disturbance are secondary to the local inflammation, and are proportionate to it" . . . Very erroneous opinions respecting pyæmia have gained admission into some of our most important text-books . . . What we call pyæmia in all its more typical forms is due to phlebitis . . . "In what has been known as puerperal fever I have no doubt that phenomena, precisely analogous to pyæmia on the one hand and septicæmia on the other, will find their respective places."

Dr. Richardson.—The woman after delivery is physiologically in a peculiar position. Her blood is in a peculiar condition: the fibrine is in excess and "in trembling equilibrium, ready on the slightest possible disturbance to be precipitated." Then, there is a diminution of the salts in the blood favourable to the precipitation of colloidal fibrin. The woman is in the condition of a person who has lost a limb: the blood-supply to the child cut off. She is in an enfeebled nervous condition . . . She is, therefore, in the exact condition for a series of changes which must necessarily be febrile in character . . . We must accept the fact that there are a considerable number of women who are hereditarily predisposed to particular diseases including puerperal fever . . .

. . . I presume there is no such thing . . . as a case of delivery which is not followed by some slight febrile state, for that state is necessitated by the changed physiological conditions, by the increased tension of the vessels . . . There is always this simple surgical fever . . . Barring that natural febrile state which follows upon confinement . . . there is no such disease as specific puerperal fever.

. . . The presence of organic germs and bacteria is a mere matter of coincidence . . .

Antiseptics, that is, bodies which prevent putrefaction, . . . antiseptics do not act by destroying germs or organic forms, but by interfering with the poisonous action of the septinous material which produces the fatal disease.

Dr. Robert Barnes, Obstetric Physician of St. George's Hospital, said he would divide the cases of puerperal fever into two great classes: (1) Heterogenetic, the direct result of infection or contagion produced by some zymotic poison, as scarlet fever, erysipelas, measles, or typhoid. All those things we see and know and cannot for a moment dispute them; (2) Autogenic, in which the conditions of the fever exist or arise in the patient's system, with which infection or contagion from without has nothing to do. Such cases are manifest to every one . . .

How is it that lying-in women are specially prone to scarlet fever? . . . How is it that the protection of a previous attack all of a sudden breaks down under the trial of child-birth? . . . "I have seen cases traced to scarlatinal poison in which the usual symptoms of scarlet fever were absent: no particular sore throat, no swelling of the glands, no rash, and yet the cases have gone on to a fatal issue."

If we look at what a lying-in woman is we there see a peculiar constitution, ready to receive poisons. . . . Then there is the peculiar condition following labour where the system has been loaded with matter . . . a state which is just treading on the verge of fever; at any moment the slightest excitement, or the slightest noxious matter carried into the blood, is ready to ferment and set up a fever. It does not matter what the poison is . . . With regard to scarlet fever, it is enough to set up any mischief in a lying-in woman, and produces all the mischiefs of any other form of poison . . .

Passing to the Autogenetic cases: these cases are as distinct in their origin as many cases of infection . . . a little bit of placenta retained, a clot of blood . . . the whole system is in a ferment just as it was from the poison of scarlet fever . . . I believe the infection may be propagated by the breath of a medical attendant or a nurse . . . A man may walk about charged with infectious disease, and those who are susceptible, with whom he comes in contact, may catch it . . . if a patient be in the lying-in state, with the blood ready to ferment, such a person would be ready to be attacked . . .

There is a pure puerperal fever which the patient herself can generate.

The spread of the disease can be prevented by careful isolation of the patient . . .

With regard to the value of antiseptics, to keep a hospital free from puerperal fever is an extremely difficult matter . . . You cannot keep a series of patients in a hospital isolated in the proper sense of the word. You have the same nurses going to and fro . . . The consequences of the poisons acting upon one or two patients "may be radiated to others." Only one secret for safety: have the woman confined at her own home . . . A lying-in hospital is not now by any means so serious a matter as it used to be: still, it is always like sitting on a volcano which will explode at any moment"!

Dr. Squire had never met with the alleged febrile condition in lying-in women: he thought the idea was derived from the depths of Dr. Richardson's inner consciousness, not as the result of bedside experience . . .

"While admitting a large class of infections associated with puerperal fever, I hope to show that the whole class of acute specific diseases may be set aside . . .

I assert that not only is puerperal fever not typhus, typhoid, smallpox, measles, diphtheria, or even scarlatina, but that these diseases are little modified by the puerperal state . . .

. . . When we come to erysipelas the case is very different . . . I do not mean to say that puerperal fever is erysipelas . . . but it has a close relation to erysipelas, to hospitalism, to purulent infection, and to suppurating wounds . . . I believe we can connect puerperal fever closely with that class of infectious diseases which the investigations of "Billroth and Lister" have enabled us to control. I believe there is danger in those dissecting or dressing surgical wounds attending midwifery cases.

Dr. Brunton, a general practitioner, felt called upon to rise and "make a few remarks." He had dissected, and made postmortem examinations, and gone on with his obstetrical practice all the time, and he had not had in the whole course of his practice a single case of puerperal fever; and then followed a self-contradiction.

It was of this class of observer among his opponents that Semmelweis said: "A blind man does not see colours, but that is no proof that colours do not exist."

Dr. Swayne, Physician-Accoucheur to the Bristol General Hospital, referred to a statement which had appeared in the "Times" to the effect that a medical practitioner who has had a case of puerperal fever ought to retire from practice for two or three months, as having created a panic, and he gave an illustration from his own recent experience.

As to the mode in which the infection of puerperal fever was conveyed he believed that some men absorbed, and then exhaled the poison, from their skin.

With regard to the precautions to be taken to prevent the spread of puerperal fever, he did not think it was necessary for a medical man to seclude himself for more than a week from midwifery practice . . . probably by that time the poison will have passed out of the system. "Immediately after seeing a case of puerperal fever I go home and, before going to bed, take a warm bath and wash myself with carbolic soap, and on the next day I take a Turkish bath" . . . "We should be very careful not to wear the same clothes."

Dr. Graily Hewitt, Professor of Midwifery in University College, considered puerperal fever "essentially a form of blood-poisoning" . . . it is a form of pyæmia. He referred to the late Sir James Simpson's teaching to the same effect, and continued: "What I have seen since has led me to endorse that view of the matter in the strongest manner possible."

There are two classes of cases of puerperal fever: (*a*) Those in which there is evidence of the introduction into the system from without of a morbific animal poison; (*b*) cases which do not resemble these, in which the evidence is wanting of the introduction from without of such a morbific poison.

In the first class, the infection is conveyed by the medical man or midwife by means of the hand: "and I believe that the spaces beneath the nails and under the skin, which covers the nails are extremely liable to harbour these destructive animal products" . . . "It seems to me rather unlikely that the clothes carry infection so readily" . . . very great attention should be bestowed on cleansing the hands . . . any animal poison introduced from without may produce what we term puerperal pyæmia, the same as may be produced by a student who is handling surgical wounds in the hospital and attending midwifery cases . . . Poison may be introduced from without in case of laceration of the perinæum . . .

The class of cases corresponding to what Dr. Barnes calls autogenetic, is illustrated by use of sponge-tents in the non-pregnant. Concurrently with the commencement of the atack of puerperal pyæmia *the involution of the uterus is absolutely retarded.* The contractile power of the uterus fails in some cases after labour: the expulsion of *débris* ceases, and there occurs a suction action of the uterus by which the *débris* is taken up into the circulation.

Mr. Callender, the surgeon, was present as a visitor. Like Billroth, of Vienna, he was opposed to Lister, and referred to the fact: he was good enough, however, to admit the value of antiseptics. His speech was merely a commentary on the discussion, and on the analogy between puerperal fever and surgical wounds.

In the discussion continued on the 2nd of June, Dr. Arthur Farre made the opening speech. There is nothing in this lengthy address worthy of attention now, except the references to Dr. Kirkland's essay on *Childbed Fever,* published in 1774. "Having asked your attention to the works of this author, and shown how far his idea seemed to be in a measure coincident with those which are now under discussion . . ."

Dr. Savage and Dr. Wynn Williams, of the Samaritan Hospital for Women, had little to add in elucidation of the subject. The latter had never had a fatal case of puerperal fever since beginning to use iodine as a disinfectant twenty years ago. "As to bacteria—a delusion and a snare"!

Dr. Playfair, Professor of Obstetric Medicine in King's College, commented on the course of the discussion up to his own address, and referred to the "remarkably little reliable knowledge we have about the subject on which we are talking." He did not believe in a miasma arising from the puerperal patient: there was no evidence to show that there had ever been an epidemic of puerperal fever . . . "I believe that the theory which considers the so-called puerperal fever to be practically the same disease as surgical septicæmia or pyæmia . . . is the one which is most consonant with the facts of the case; that it arises from the contact of septic matter with lesions of continuity in the generative track, such as exist in every parturient woman.'' It is obvious from further remarks that Dr. Playfair had not yet quite shaken off the contagionist faith in the influence of zymotic disease as a factor in producing and modifying puerperal fever, but his opinions otherwise were evidently derived from the Edinburgh School.

Dr. Tilt said if the zymotic influence is to have ascribed to it such an extraordinary effect in the production of puerperal fever, scarcely a puerperal woman in London could escape . . . If too much importance has been attached to zymotic influence . . . too little has been attached to the autogenesis of puerperal fever. ''I think that if a woman be sometimes poisoned by others, she more frequently poisons herself.''

Dr. Fordyce Barker had come from New York specially for the debate in order to press his well-known peculiar opinions about puerperal fever once more upon the attention of the medical profession. After much verbiage he said: ''I will now give my reasons for believing that there is a disease which may be properly called ''puerperal fever'' . . . the disease is an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid.'' Among the propositions which have a sort of interest in the history of medical error there is the following: ''The clinical phenomena of puerperal fever are quite different from those which are met with in surgical septicæmia or pyæmia.''

Dr. Charles West, speaking of microscopic organisms as a factor in the production of puerperal fever, ''we have not as yet a sufficient amount of knowledge on the subject to be able to apply it, or to draw from the observations that have already been made any correct and useful inference.'' He did not see that we have advanced far, if at all, beyond the conclusions which Dr. Ferguson laid down, that the phenomena of puerperal fever depend upon a vitiated state of the fluids, and that in the case of a woman in the puerperal state, such vitiated state of the fluids is specially apt to arise . . .

. . . It seems as if the puerperal state itself was the condition of the development of this set of symptoms . . . None of us can describe at all wherein the alterations of the blood consist.

Dr. Snow Beck's speech consisted of a commentary on what had already been said in the discussion. He thought it is worth while to controvert the opinion that puerperal fever was a disease *sui generis* . . . "Dr. R. Ferguson, in his admirable essay on "Puerperal Fever," wherein he correctly attributes the most serious diseases to a vitiation of the fluids, *though the nature and source of this vitiation is not so clearly stated as perhaps it might have been."*

One expression of opinion may be quoted as curious and interesting in marking a stage in the evolution of the pathology: "Laceration of the perinæum may exist at the same time as want of contraction in the uterus, but it is the want of contraction in the uterus which admits the septicæmic condition of the system to be induced, not the laceration of the perinæum " Hysteron proteron: the cart before the horse!

Dr. Routh recalled his experience at the Lying-in Hospital of Vienna in 1846 and 1847, and bore testimony to the results of the Semmelweis discovery—"One point in regard to the disease was clearly brought out—namely, that it was not contagious from one person to another."

Routh was not the young and inexperienced man of the Medico-Chirurgical Society Meeting in 1849, and it is disappointing to find a certain suggestion of retrogression in his opinions. Under the home-influences he has ceased to hold clear views about the effects of the zymotic diseases in puerperal women . . . "Some cases as not contagious and some are." He should have remembered amidst the floods of self-satisfied ignorant verbiage and reserved clear scientific statements of truth to which he was listening, that under the principles of Semmelweis no disease could be the cause of puerperal fever unless it produced a decomposed animal matter which must be conveyed direct to the healthy puerpera.

All that was true in this discussion was in accordance with the principles of the Semmelweis Doctrine: everything else was erroneous.

HEGAR'S CRITICISM.

Concerning this discussion, Hegar says in a long Note to his "Life and Doctrine of Semmelweis" some things which make painful reading for the British gynæcologist. Much that he says must be regretfully admitted to be too true, but to the writer and his countrymen we would submit "with all respect," as the barristers say to the judges on suitable occasions for remonstrance, that Hegar exhibits a certain national bias which ought to be foreign to science. Science is, or ought to be, cosmopolitan. An amiable feature of the bias is shown in a remarkably mild and indulgent tone when referring to some of the bitterest antagonists of Semmelweis, whatever their misconceptions and errors. We must admit that some obstetric troglodytes disported themselves in the debate of 1875, and there were "to the fore" several obstetric mandarins, our equivalents for Scanzoni and Levy and Litzmann and Carl Braun; but due consideration is denied to the school of Simpson which was well represented in the discussion, and better still in the contemporary medical journals available for Hegar's perusal.

Germany even at that time had no equivalent for the Edinburgh School.

"How long," said Hegar, "antiquated notions are retained, not merely in popular tradition but in the heads of the medical profession, even in those of the leaders of a department of medicine, is very well illustrated by the *Transactions of the Obstetrical Society of London* in the year 1875. Here we find an olla podrida of etiological opinions, a collection of the newer or quite antiquated views, the last reflected or somewhat modified in a more modern brain. According to some a chill, or emotional excitement, is still capable of causing *febris puerperalis*, or at least of representing the chief etiological factor. The body of the puerpera is loaded with matters which must be expelled. If anything hurtful in any way intervenes, which in another person would remain absolutely innocuous, if a feverish condition arises from any other cause whatever, then the mine explodes. In the trembling equilibrium in which the blood and fluids exist, it requires only some disturber of any sort, and the devil is let loose. Even a quite spontaneous origin of the malady is possible, if it occurs that the tissue elements, which are absorbed during the involution of the uterus, are not eliminated with sufficient rapidity. Barnes has expounded this doctrine in more detail so late as 1882 in the *American Journal of Obstetrics*. In these *Transactions* we find it taught that the poison of scarlet fever conveyed to a puerpera causes in that person *febris puerperalis*, and, when carried from this individual to another, produces characteristic scarlatina. " Septicæmia may result from an injury without any infection being conveyed, merely because the person affected is in an unsatisfactory state of health. Even all infectiousness of *febris puerperalis* is denied. A few of the speakers even held firmly to the belief that puerperal fever is an essential fever which can affect only women in the lying-in state."

"Whoever will take the trouble to look through these *Transactions*, a thing which I would not however recommend, will find other remarkable things of a similar sort. I have called attention to those mentioned in order to show in what confusion (*Wirrsal*) the doctrine of puerperal fever still remains beyond the Channel.

" It has been generally believed that what Semmelweis taught had been known for a long time in England. Now I believe that no one who knows the *Transactions of the Obstetrical Society*, and also what was formerly produced in England concerning the relations of the specific infectious diseases to puerperal fever, and the contagiousness of the latter malady, will confuse the simple and clear doctrine of Semmelweis with these distracted (zerfahren) and unsettled opinions."

Here we have a weighty expression of opinion which we gladly welcome as beneficial. But few in England ever did confuse the "simple and clear" with the *zerfahren* until claims began to be advanced many years later for Oliver Wendell Holmes as the discoverer of the etiology of puerperal fever.

MATTHEWS DUNCAN. Although Matthews Duncan could not take any part in the Discussion on Puerperal Fever at the Obstetrical Society, his opinions are interesting as being the clearest and most advanced of that time, and it might be added, truest, as proved by the subsequent experience of over thirty years.

A contribution to the science of the subject had appeared in the *Obstetrical Journal* in September, 1874, entitled "On Puerperal Pyæmia, etc.," by J. Matthews Duncan, Physician to the Royal Maternity Hospital . . . . Edinburgh. The following is a summary.

The term 'pyæmia' may be considered identical with septicæmia and ichorrhæmia employed by some. Our knowledge has been retarded in its growth by the way in which autopsies used to be made—not the work of specialists in pathology, and therefore worthless. Now an autopsy is a matter understood to demand the labour for a long time . . . . of an expert.

In some acute and rapidly fatal cases of septicæmia no postmortem appearances were made out, except an alteration of the blood with enlargement and degeneration of the spleen, etc. This used to be an argument for the essential character of the disease.

van Swieten, Willis, and many old writers on puerperal fever, regarded the malady as a wound-fever, and Eisenmann's work, published in 1837, is called "Wound-Fever and Childbed Fever." The wound they had in view was that produced by the separation of the placenta, but now we recognise that the wound may be a laceration, contusion, or fissure anywhere in the genital canal. . . .

Following an as yet indefinite conception of wound-fever, came a further true advance in the demonstration of the identity of the morbid anatomy in patients dying after surgical and after obstetrical wounds. This step we owe to Cruveilhier, Simpson and others. Next came the discovery and description of phlebitis and lymphangitis, a grand piece of progress, which we owe chiefly to Cruveilhier, Robert Lee. . . .

A still further step in establishing the nature of puerperal fever was the discovery of thrombosis and embolism by Virchow, Kirkes, Cohnheim and many others.

Next came the researches into the potency of septic poisons—researches as to the production, diffusion, and the influence of bacteria, which we owe to Lister, Klebs, Billroth, Heiberg, Orth, and many more.

Great credit is due to Semmelweis for the good he has done, especially to hospital patients, by his enlightened zeal in the cause of prevention; but much has yet to be accomplished. Prophylaxis is still farther to be carried out . . . . by disinfection . . . . subjects all at present receiving attention from the profession.

The most important researches on bacteria have been those of Lister and his followers, undertaken with a practical object in view. The results have been equally wonderful and valuable. These results go far to justify the belief that pyæmia is a septic disease, and that puerperal pyæmia may be almost if not altogether prevented by the application to delivery of a practice based on antiseptic principles. . . .

The rules of Semmelweis, or any other washing of the hands, do not secure cleanliness according to Lister, nor constitute Listerian treatment. . . .

We have yet a long way to go in order to secure complete antiseptic delivery.

THORBURN. In the year 1875, when the attention of the medical profession of the United Kingdom was turned upon the discussion in the Obstetrical Society of London, many articles and papers on puerperal fever appeared in the medical journals, largely from members of the Society who were unable to take part in the discussion.

Among such contributions was one "On Puerperal Pyæmia,"[1] by Dr. Thorburn, Lecturer in Obstetrics and Gynæcology, Owens College School of Medicine, Manchester. We select this contribution as an illustration of what was then really taught concerning puerperal fever in England, and the author as the best type of enlightened provincial teacher of midwifery. Dr. Thorburn received his medical education at Edinburgh on a solid basis of mental ability and sound sense. His career as teacher was spent in the capacity of professor of midwifery in the most important provincial school of medicine in England. It began as Assistant to Simpson about 1856.

He was probably the first in England to introduce Listerism into gynæcological surgery. He was incapable of modifying his professional judgments with a view to personal advantage or popularity, and he did not possess a trace of that worst of all faults in a teacher, an affectation of eccentric or exclusive opinions put on for the sake of notoriety and its reward. We are surely safe in selecting Dr. Thorburn as the type of provincial teacher of midwifery of his time. He was typical also in his disabilities: he had no means of giving clinical instruction.

Let us look into the principles on which he founded his teaching on puerperal fever. Does he rise to the Hegar standard? He says: "The puerperal woman is placed much in the same position as one who has undergone a serious surgical operation, involving raw absorbing surfaces. . . . She is specially liable to attacks of infectious disease in consequence of this wound and lowered vitality. . . . She is also most liable to every form of pyæmia and septicæmia, that is, puerperal fever. . . . Puerperal fever, however induced, has a remarkable tendency to be conveyed to other puerperal women. . . . To prevent this it is therefore the duty of every practitioner to avoid as far as possible any communication of effluvia, secretions from erysipelas, pyæmia, unhealthy sores . . . to his obstetric patients. This implies that he shall . . . use antiseptics, change of clothing . . . it cannot possibly imply entire abstention from visiting infectious cases while doing midwifery work. The occurrence of a case of septicæmia calls for redoubled precautions. He agrees on principle with Matthews Duncan that quarantine may be necessary. . . . " A professed obstetrician should not indulge in post-mortem examinations or other probable sources of septicæmia."

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[1] *Brit. Med. Journal*, June 12, 1875.

We have here stated opinions and practice as modern and scientific as any to be found in Europe then or now. We can discover some trace of the contagionist creed; but it is a faithful reflection of the teaching of Simpson of Edinburgh and the Edinburgh School of Medicine modified by the writer's experience and individuality; and whatever Hegar and some Continental authorities might say, it was the most consistent and efficient practice in Europe of that time, founded on an etiology and pathology which have stood the test of time.

LEE. Of very different quality from the paper of Dr. Thorburn was that which was published by Dr. Robert J. Lee, Assistant Physician at the Hospital for Sick Children. The subject was puerperal fever and it was published in the *British Medical Journal* of May 13th, 1875. Lee was not a teacher of midwifery.

This is perhaps the last belittling attack on Semmelweis which ever appeared in any British or Continental medical journal. The writer compares Gordon, of Aberdeen, and Semmelweis very much to the advantage of the former; but he shows that he has not even yet, in 1875, been able to distinguish between the old obsolete doctrine of the contagionists and the modern and thoroughly established etiology of puerperal fever as wound-fever.

"The method of investigation pursued by Semmelweis was much the same as that followed by Gordon."

That is true if "postmortem examination" always implies the same thing, apart altogether from the way in which it is made, and whether by amateur or expert.

It was only the year before that Matthews Duncan, in his paper on Puerperal Pyæmia, expressed something like contempt for the amateur postmortem work which had obstructed the progress of the science of puerperal pyæmia. That Gordon was a very awkward amateur must be the conclusion of every one who has read his own account of his work with reasonable attention.

Dr. Lee further declares that Semmelweis gives proofs of possessing very limited knowledge indeed of the literature of his subject, and he consequently finds very little difficulty in disposing of his opponents to his own satisfaction. Such language is beyond criticism: it is merely utterly prejudiced, wrong-headed and exasperating. It is because it is unique in the English literature of the subject that we have thought it worth while to call attention to Dr. Lee's contribution, not from any misconception as to its quality.

It reads like the work of a man who had been to Vienna, and obtained his information orally in Carl Braun's lecture-room.

VIII.

FORERUNNERS AND CONTEMPORARIES OF SEMMELWEIS.

The discredit of the differences of opinion with some obscurantism prevalent among even the teachers of obstetrics in England so late as 1875, implied in Hegar's sarcasms, is not so obvious when we come to examine closely into the historic causes and analyse the facts. It was not so discreditable as was the unanimity among the professors of midwifery in France and Germany and Continental Europe generally up to about the same time. In England the differences arose chiefly from independent thought and observation, influenced here and there by the adoption of irrational opinions from France or Germany, and modified by the genius of the individual "authority." In Germany, on the other hand, the absence of differences concerning the fundamental principles at the basis of the etiology of puerperal fever, was owing to the universal acceptance of the doctrine of the *genius epidemicus,* the atmospheric, cosmic, telluric influences. It is difficult to grasp even now the position: it seems impossible that educated thinking men should have for generations acted in the most serious concerns of professional life on such irrational, unthinkable notions, simply because they were taught by the masters and by them of old time. Upon the whole bodies of men have in religion, politics and medicine always accepted the *verba magistri,* they do now, and they always will do; but in the history of medicine there is no more remarkable illustration of universal suspension of judgment concerning matters of such immeasurable importance in medical science and practice, that is to all human communities, as that concerning puerperal fever. In the field of religious dogma there had appeared a Martin Luther and less fortunate reformers; in physical science a Galileo, in biology a Servetus, and in politics a legion of martyrs have been sacrificed to their faith in the battle-field or on the scaffold in all ages. In the history of obstetrics there has been only one Semmelweis, and he did not appear till the middle of the 19th century: it ill becomes the fellow countrymen, who crucified him before accepting his doctrine, to sneer at the minor defects in the medical science of a people who in essentials had left them far behind generations before.

The forerunners of Semmelweis were almost entirely to be found in the United Kingdom of Great Britain and Ireland. The fact is too obvious to require discussion, but the causes are of great interest, and might be readily traced if space permitted. To state them in the most concise manner is a very difficult task.

The first of the important causes of the pre-eminence of British obstetricians was the early and successful assertion of the claim to practice midwifery by medical practitioners, as against the traditional usage and the strident demands of women for the perpetuation of the exclusive privilege of attending women in labour.

FRANCE. In France the domination of the *sages femmes* was the first to be established and the last to be displaced. Most British medical readers, who as students have been made familiar by professors' quotations with the names of Mauriceau, Palfyn, Levret, Baudelocque, Dubois and many others up to the present generation, have taken the impression that these French obstetricians were distinguished men whose endowments and experience had placed them in the position of consultant specialists, and occasional advisers of many obstetric medical practitioners in their more formidable difficulties. As a matter of fact such men only came to the aid of the *sages femmes* when called in, and the most important of them were not even permitted to cross the threshold of the old Hôtel-Dieu or the modern Maternité except when sent for by the personage who occupied the position of head-midwife.

When Boër visited Paris in 1788 he could not gain access to the lying-in hospital except for the influence of Marie Antoinette, exercised in his favour through the Austrian ambassador. It was the same with Osiander early in the 19th century; and Arneth, whose visit occurred so late as 1850–51, gives an interesting account of his experiences in the matter of admission.

" For foreign medical visitors it is almost an impossibility to see the Maternité. More than once it has been found, even by appeal to diplomatic influence, impossible to obtain permission to visit the Maternité more than once. . . . Even the members of the staff of the institution have to make a formal application to the lay directors for permission to take a visitor with them into the Maternité. . . ."

"As a school for midwives, Arneth considers the Maternité unique in the world (*Vielleicht einzig in der Welt*) in its efficiency. . . . And this great institution is closed to students of medicine, so that in the 19th century in the capital city of France, there is no midwifery clinic for medical students. Midwives establish small maternity hospitals, and give theoretical and practical clinical instruction in them, including the performance of obstetric operations. In 1831 v. Siebold saw a midwife's advertisement to that effect. The pretext for the hermetic closing of the Maternité was a regard for decency. This in Paris and in the country which had witnessed the orgies of three revolutions!

It was only in 1834 that a small lying-in hospital was opened for the clinical teaching of students of medicine, and it was placed under the same chief, Dubois, Director of the Maternité. v. Siebold, in his " History of Obstetrics," says: " Midwives continue to give practical instruction to the students of medicine. Access to the Maternité, accorded to the students, can be the only means of remedying this great defect . . . and rousing in France the love of obstetrics . . . by which alone the progress of obstetrics can be assured." (*Herrgott's Trans.*, Vol. ii, p. 655.)

In a country where the ablest men in the capital could obtain no opportunities of prolonged and exact clinical observation of the individual case, and the provincial practitioners had no independent midwifery practice at all, it was hardly to be expected that any progress would be made towards solving the supremely difficult problem of the etiology of puerperal fever. Dubois at the time of the revolution of 1848 had no more real knowledge of the subject than had Ambrose Paré in the time of Henry II.

GERMANY. In Germany the state of midwifery teaching and practice in the 18th century was even worse than it had been in France. Men were not permitted to attend women in labour. Was not an enterprising doctor burnt at the stake in Hamburg in the century before for attending in disguise a case of labour? There was no instruction for midwives except by oral tradition from old women to young—old wives' fables.

Owing largely to the paralysing effects of the Thirty Years War, Germany did not participate in the progress made in France. Medical men were still excluded from obstetric practice, except when requested by midwives in extreme emergencies, and hence it arose that the surgeon was associated in the popular mind only with obstetric atrocities and cruelties. So there was added to the excluding influence of alleged modesty, popular fear and aversion.

Medical men gradually began to give instruction to midwives, but the teaching was purely theoretical and had to be conveyed by the simple reading of manuals, and this custom held its sway till well into the 19th century. It will be remembered that one of the counts against Boër of Vienna was that he refused to be tied down to the reading of an official manual to his students. He must assert the right to draw upon his own knowledge. There was no lying-in hospital anywhere in the country; the French, on the other hand, had the maternity portion of the Hôtel Dieu since 1664.

Every influence at work had a tendency to magnify the office of midwife and degrade the medical profession. The Duke of Saxony's ordinances for midwives, published in 1682, were just a little in advance of the Rules of the Central Midwives Board in England prepared in 1903. Even such an appointment, as that of Justine Siegmund to be midwife to the Court of Brandenburg, raised the position of the midwife and lowered by comparison that of the surgeon, who was still excluded from the ordinary practice of midwifery. "Die Siegmundin" wrote a manual of midwifery, which was probably useful, and she was imitated in this respect on a great scale by midwives and surgeons alike, but everything obstetric remained, as v. Siebold said, in a state of "notorious inferiority" for another century.

The first Obstetric Clinic for students of medicine in Germany was started at Göttingen in 1751 by Roederer, a pupil and disciple of Smellie, for whom he shewed his admiration by translating and publishing his book without acknowledgement. In the same year a sort of school for midwives was founded at the Berlin *Charité*. The pupils then received formal if not systematic instruction from the doctors, and had to pass an examination of a kind. It was not till about 1780 that a course of midwifery for surgeons was commenced in Berlin. and no Clinic for students of medicine existed there until 1817. Midwives still held the upper hand.

Since the beginning of the 19th century a revolution has occurred with regard to the teaching of midwifery in Germany. All the universities now have lying-in hospitals, and the clinical teaching for several decades has been admirable.

With regard to the training of midwives great improvements have been introduced. Suitable pupils are in a certain measure selected. They have to pass a real examination, and in their practice they are each of them assigned to a district and are under a modified medical supervision and control. There are excellent manuals of midwifery for midwives available in every State; many old abuses and prejudices have disappeared.

Old traditional usage and modern improvements have given the German *Hebamme* a large amount of popular consideration, in contrast to the English usage of 150 years. About 90 per cent. of all cases of labour are still conducted by midwives in Germany. The medical profession has never sufficiently asserted its claims, and has consequently never been granted the opportunities which are a commonplace privilege of the general practitioner in England.

Considering the assured position of the general practitioner upon whom in large measure has depended the advancement of obstetric science in the United Kingdom for a century and a half, the introduction of regulations for midwives is a thing devoutly to be wished by all benevolent Englishmen, lay or medical. In Germany the claims of the general practitioner have yet to be asserted; in England he has held the upper hand too long to fear rivalry.

AUSTRIA-HUNGARY. The history of midwifery in Austria and Hungary is analogous in every respect to that of Germany. The causes of the backwardness of the teaching and practice of midwifery by medical practitioners were almost identical, political disturbances and social disorganisation resulting from feud and open war. The beginnings of amelioration were also identical, the exercise of a benevolent autocracy by the sovereign of the period.

Improvements in Vienna were, as we have seen, begun by Maria Theresa when, after many struggles, she found her diminished empire at peace.

v. Siebold says: "The great States gave a salutary example in the teachings of midwifery; it was thus that Maria Theresa, that noble sovereign, selected . . . . *Crantz,* a disciple of the great *van Swieten,* and sent him to Paris and London to follow with ardour the eminent masters who taught midwifery with the object of fitting him to teach in his own turn on returning home.''

Crantz returned to Vienna in 1754, and set to work to raise the standard of midwivery *among the midwives*, and to make a beginning with the teaching of midwifery to medical students and surgeons. Crantz wrote much for the training of midwives, and we have seen what an important position the School for Midwives occupied in the General Hospital at a later time. We have here one more illustration of the elevation of the midwife and the relative degradation of the doctor—all in contrast with the system which had long become established in England.

In Hungary as was to be expected on the verge of the barbarous East, medical education in all its forms was for long at a low ebb, but there was an awakening early in the 18th century. How slow was the evolution of the Science and Art of Obstetrics we have seen indicated in the arrangements of the Clinic at Buda-Pesth when Semmelweis was professor. In his class of midwifery at one time he had 68 medical students, paying no attention because midwifery was not a compulsory subject for the degree examination, and 199 midwife-pupils who could not quite understand the ill-developed technical terms, and perhaps the teacher's particular dialect, of the Hungarian language.

As indicating the state of midwifery practice in Hungary in the last half of the 18th century, v. Siebold quotes a dialogue between the Emperor Joseph II., for whom there was nothing too great or too small if it affected the welfare of his subjects, and the Hungarian teacher of midwifery, Werzpremi, who had been a pupil of Smellie.

To the question of the Emperor, ''Exercesne tu quoque hic illam artem, et quo successu? Werzpremi replied, giving expression to the usual pretext of wounded modesty: Fatendum est ingenue, Augustissime D. me rarius ad parturientes vocari, ita enim sunt pudicae mulierculae nostrae, ut mares non facile admittant, nec patiantur sibi a viris auxiliatrices manus admoveri." To this the Emperor replied: Utinam non essent adeo pudicae! (Herrgott II., p. 387). Which may be strongly translated: "I wish to God they were not so modest."

In a country in which midwifery was not a compulsory subject for a degree examination, and where such sentiments prevailed among the people, what progress could be expected even in the mere mechanical part of obstetric practice to say nothing of clinical observation and intellectual acumen required to establish the true etiology of puerperal fever. The science of obstetrics could only be imported from the West.

Italy. Italy has in comparatively recent times contributed much to the advancement of obstetrics and gynæcology, but largely, if not exclusively, on the surgical side. To Italy we owe Porro's Cæsarean section, and much more in operative midwifery.

If we examine such a work as that of Corradi, *Dell' Ostetricia in Italia dalla metà dello scorso Secolo fino all' Presente* (Midwifery in Italy from the middle of last century to the present time), published at Bologna in 1877, we find only the same story of backwardness in midwifery instruction and practice, such as we have seen in Germany and other Continental countries, and from almost identical causes—the division of the Italian people into many small states, the pre-occupation of the men with feuds and fights, when there was no actual civil war on hand, neglect of education, and the position assigned to women in connection with child-birth and the health of ailing women generally. There was no employment in obstetrics for the male intellect.

The foundations of possible progress were laid by Maria Theresa, who commanded (*ordinò*) that a school of midwifery for midwives should be instituted in the Spedale Maggiore of Milan in 1767. Among the early instructors there we find the name of Assalini.

Progress, though slow, ultimately became visible throughout the country. The sequence of events was much the same in every Italian state: a school for midwives at an old university, then theoretical teaching to young surgeons, and at last a small clinic established in some old religious house, taken over or bestowed by the authorities for the purpose.

In Rome, for example, a school of midwifery was not commenced till 1786: Asdrubali, like Semmelweis, taught both students of medicine and midwives, and he and his assistants occasionally visited Paris to learn the most recent changes or improvements in the theory and practice of obstetrics. It was in some measure this process of professional intercourse which led to scientific progress, while fixing the preconceptions of the older schools upon the younger. Italian obstetrics borrowing from France became epidemicist in its doctrine of puerperal fever.

In illustration of how little opportunity was granted in the medical schools of Italy for the prolonged observation and generalisation such as are required for the elucidation of such difficult subjects as puerperal fever, it may be mentioned that Genoa did not obtain an obstetric clinic until 1834, and it was not until 1852 that midwifery was entirely separated from surgery both in teaching and hospital arrangements. Owing to the domination of the midwife a century after she was almost superseded in England, and the defective education of the medical student, nothing was to be expected from the general practitioner towards solving any obstetric problem even of the simpler sort.

In the portion of Corradi's vast work which is devoted to puerperal fever (*Della così detta Febre Puerperale*) we find a review of all the ancient theories. From early in the 19th century we trace the influence of the doctrine of the constitutional causes of local diseases of which the chief British authority was Abernethy. For example, Fasola, in 1811, not quite emancipated from the milk-fever doctrine, "la febbre mostrando sempre tendenza a render locale la malattia con morbosa mutazione nel systema nervosa." All which is about as thinkable as " atmospheric cosmic telluric influences."

There is much quotation of authorities, ancient and modern, in Corradi's work, and we occasionally get a glimpse of our modern doctrines through a mist of obsolete opinion. Eisenmann's doctrine of phlogistic and erysipelatous wounds, of which Pouteau of Lyons is said to have been the originator in 1766; and "we ought to mention," along with Pouteau the aphorism of Hippocrates: " Mulieri praegnanti erysipelas in utero lethale." And bringing us a little nearer the doctrine of wound-fever we have a quotation from van Swieten (1765) from the aphorisms of Boerhaave: "Uti in vulnere, ita et in puerperis, illo praecise tempore febricula adest." What now of the priority of Cruvheilier according to Hegar?

Juncker is described in Corradi's work as genuinely the first to declare puerperal fever as wound-fever: " Puerperae tamquam vulneratae merito considerantur."

Then coming to recent times, we find Padovani's theory of thrombosis as the *cause* of puerperal fever expounded, and we are assured that the arrest of involution, which we have long recognised as produced by sepsis, was the sole cause of puerperal fever ("*Che la metro-paresi sia l'elemento generatore quasi exclusivo della febbre puerperale* ").

It is not till 1862 that we find Tibone's references to puerperal fever observed in country districts, just like the observations by English medical practitioners which had been going on then for a century, and had formed the clinical foundation for the doctrine of contagion. And in Italy we also discover " l'occulto quid divinum," the " unknown something," the " unbekanntes Etwas," the elusive entity which left every explanation of the etiology of puerperal fever not perfectly complete.

In the whole chapter on Puerperal Fever in the work of Corradi the name of Semmelweis does not once occur.

Holland and Denmark. Midway between the French and German belief in a *genius epidemicus* as the fundamental etiological factor in puerperal fever and the British doctrine of contagiosity stood a set of vague opinions which could not be clearly enunciated as either one or the other. Such were the rather illogical and even contradictory but practically useful doctrines to which we have had expression from Tilanus, of Amsterdam, in reply to the letter of his pupil Stendrichs announcing the Semmelweis discovery.

We have seen that Michaelis, of Kiel, received the tidings from Vienna, just as the English obstetricians and teachers of midwifery in London accepted the Semmelweis Doctrine from Routh, with minds prepared to receive it, and they at once put it into practical application.

Levy, of Copenhagen, was a man of a different type. He had not the intellectual acumen of Simpson, of Edinburgh, to appreciate the difference between Semmelweis and the contagionists, even when it was pointed out to him, and when the difference was explained, he had not the candour to admit, like Simpson, that he had been wrong. Levy had just returned from visiting the British lying-in hospitals when he received a copy of the Vienna letter from Michaelis, and he replied that he knew all about the etiology of puerperal fever, even before he heard the name of Semmelweis. Still, in spite of some pedantic objections to the Semmelweis Doctrine, he applied the method of prophylaxis, as did the colleagues at Amsterdam and at Kiel, so that next to Great Britain, Holland and Denmark adopted most frankly in practice the Semmelweis doctrine of conveyed infection as distinguished from contagium.

The more the people of Northern Europe were brought into contact with England, considering the traditional similarity of these peoples in their habits of cleanliness, the nearer did professional opinions about many things approach similarity, and in nothing was this more striking than in obstetric science and practice.

Holland has always held a leading place in obstetrics among the nations of Europe. v. Siebold speaks of Holland as drawing its inspiration from Germany, but it might fairly be claimed for Holland that the process was just the reverse. Some of the first professors of midwifery in Germany had been pupils of Smellie's, whereas the Dutch visitors of the same and a previous generation had gone to London, not as pupils, but as foreign colleagues, to see what was going on, what was new in practice, to compare opinions, and on return home to give effect to their experiences.

It was no doubt in this way that the professors of midwifery at Leyden and Groningen and Amsterdam came to hold those opinions concerning the etiology of puerperal fever which so closely approximated the views of the British contagionists, with the result that the doctrine of Semmelweis was at once understood and welcomed in Holland, as it had been in Great Britain and Ireland.

It will also be remembered that the Edinburgh School of Medicine obtained its teaching of midwifery direct from Holland: Munro the first professor of midwifery had been a student of Boerhaave at the University of Leyden. It was largely on religious and political grounds that there were, for two centuries or so, such close relations between Scotland and Holland, and so many benefactions were conferred by the most free and enlightened country in Europe upon the comparatively backward people of Scotland.

But in spite of its early start in the race of scientific progress, Holland never quite reached the stage of British contagionism, much less the new evangel of conveyed infection (Uebertragbarkeit).

The history of midwifery in Denmark has an interesting resemblance to that of Germany and Austria. In Denmark we find early in the 18th century that the practice of midwifery was almost entirely in the hands of midwives, and that surgeons were called in only in order to make use of homicidal instruments. The conservative midwifery forceps were employed for the first time in Copenhagen in 1750. The successful teaching given to midwives led to a demand for a better knowledge of midwifery by medical practitioners. Buchwald and others visited Holland in particular, and were soon able to supply the required teaching. After being hampered for a long period of time owing to the want of a maternity hospital, this defect was made good largely by the influence of Berger, who became Professor of Midwifery in 1759. The history of the rapid progress of obstetrics in Denmark from this time bears a strong analogy to that of Austrian progress by the beneficent influence of an enlightened monarch. M. Saxtorph was sent by King Friederich V. to Vienna, and all round cultured Europe in fact, to acquire such a knowledge of midwifery as would enable him to give the most advanced teaching of that time to his fellow-countrymen on his return home. Saxtorph became Professor of Midwifery in 1785, and Director of the Lying-in Hospital, now completely separated from the General Hospital. It will be observed that this arrangement was made only one year later than the establishment of the great Lying-in Hospital of Vienna by Joseph II., without separation of the Lying-in Hospital from the General Hospital. Saxtorph soon became one of the most eminent teachers of midwifery in Europe, and from his time the teaching of obstetrics in Denmark has always held an honourable place among its rivals or neighbours throughout Christendom.

It is interesting to recall the fact that it was a member of a later generation of the Saxtorph family, then Professor of Surgery at Copenhagen, who was one of the most strenuous and influential advocates of Lister's principles and practice, when the Carl Brauns and Scanzonis among English surgeons were sturdily opposing, by methods not always creditable, the principles of Lister in the United Kingdom.

In one respect there was a distinct difference between the evolution of midwifery teaching in Denmark and in

Great Britain. From the first the systematic clinical teaching of midwives was given a too prominent position in Denmark, and consequently even at the present time midwives receive an amount of consideration in their training and in the supervision of their work, hardly appreciated in the United Kingdom. The immeasurable advantage thus accruing to the working-class population of Denmark can be perhaps only appraised by those who have an intimate knowledge of the deplorable results of midwives' practice in modern England. But still what was of immediate advantage to the people was a loss to obstetric science. Denmark produced a Levy, not a Semmelweis.

Saxtorph senior was succeeded in the Chair of Midwifery in Copenhagen by his son Sylvester, who was a worthy successor to his father. Sylvester Saxtorph was succeeded in 1840 by the Professor Levy whom we have found opposing and misunderstanding Semmelweis. After his tour in England Levy became a convert to full-fledged contagionism, tempered with the ancient doctrinal obscurantism. Unlike the English obstetricians he was unable to grasp the new and fruitful idea to which Semmelweis called the attention of Europe in the *Ætiologie*.

Yet it is not to be assumed that scientific progress beyond contagionism was barred by the practice of midwives: on the contrary the work of these specially trained midwives supplied the medical practitioners with much food for reflexion. "Epidemics" were by no means of rare occurrence, but they came to be explained under the Levy influence, as in England, on the Theory of Contagion.

Some of the best illustrations, of a not too recent date, are given by Stage in 1868 in his account designated "Researches concerning Puerperal Fever in Denmark outside of Copenhagen" (Undersögelser angaaende Barselfebern i Danmark udenfor Kjöbenhavn). This account of various outbreaks of puerperal fever in small towns and villages, on the mainland and in the islands, bears a strong analogy to the contributions of English practitioners for many decades before.

We are safe to state, however, that owing to the evolution of obstetric education in Denmark the general practitioner never had a chance of contributing to the etiology and pathology of puerperal fever like his contemporaries in the United Kingdom.

v. Siebold truly says of Denmark in the first third of the 19th century: "Nothing was wanting in the teaching of midwifery in this country. We find everywhere accomplished midwifery practitioners, and recognise that all take an active part in advancing obstetric science." But in Denmark general medical practitioners as compared with the English contemporaries were too late in finding opportunities of making scientific observations upon puerperal fever.

SWEDEN AND NORWAY. While Sweden and Norway drew their teachers of midwifery from among the disciples of Saxtorph of Copenhagen, the principles on which these teachers acted regarding puerperal fever were largely those of Germany and France. In the beginning of the nineteenth century the men of mark in Stockholm were Cederschjöld, a disciple of Saxtorph, professor from 1817, and Retzius who from 1828 was director of the small lying-in hospital.

As among the opponents of Semmelweis Carl Braun mentioned with obvious satisfaction Retzius and Faye of Christiania (*Ætiologie,* p. 524). The reply of Semmelweis implies that the Swedish and Norwegian professors belonged to the same class as Carl Braun himself, the teachers of midwifery who tried the prophylaxis without conviction and earnest endeavour: "Retzius in Stockholm lost 3·3 per cent., and Faye in Christiania lost 15 per cent. of lying-in women in spite of chlorine disinfection" (*Ætiologie,* p. 526). But worst of all in the 1860 "epidemic" of puerperal fever, Retzius had to admit that in the new lying-in hospital of Stockholme the morbidity reached 40 per cent. and the mortality 16 per cent.

A University had been founded in Christiania in 1811 and a lying-in hispital in 1818. At the head of this small institution, which was devoted to clinical instruction, stood for many years Thulstrup, a disciple of Saxtorph. To him succeeded Faye in 1844, on whose management and results Semmelweis had reason to make animadversions.

In considering the barrenness of Norway in scientific contributions on the pathology and prophylaxis of puerperal fever in those days, it must be remembered that midwives were by far the most numerous students of midwifery, and they had the practice of midwifery almost entirely in their own hands. Among the reasons for this the topography of the country counted for much, in Norway as in Denmark. If there was isolating *Oe* and *Sund* in Denmark, there were *Bjerg* and *Dal* in Norway, and the doctors were few and far between.

Nowadays the Norwegian general practitioner has in his medical curriculum a prolonged and thorough training in midwifery as any student, and owing to the isolation and other circumstances of his practice he is about the most self-reliant and resourceful practitioner in Europe. But the problem of the etiology of puerperal fever was settled before this stage of evolution was reached in Norway.

GREAT BRITAIN AND IRELAND.

We have endeavoured to outline the obstacles that lay in the way of continental obstetricians towards the discovery of an Etiology of puerperal as compared with their British contemporaries, and we assign to the relative position of midwives the chief difficulty in the problem. Perhaps sufficient prominence has not been given to certain general influences in favour of the medical profession in the United Kingdom as compared with their continental contemporaries.

The chief of these was perhaps continual peace, or at least immunity from disorganisation within their own boundaries by war. Since the Revolution of 1688, there had been no disturbance worthy of mention, and the political union tended upon the whole towards progress in medical science. From Blenheim to Waterloo, Englishmen fought largely by proxy, and they ingloriously paid the costs when they did not fight. The various revolutions and the career of Napoleon almost completely arrested the advancement of every thing in medicine except military surgery. These are matters that ought in fairness to be constantly kept in mind when comparing British with Continental achievements in Medicine during that long period.

Upon the whole, as has already been said, the most striking difference between Continental and British opinion regarding the causation of puerperal fever was the rejection of the epidemic theory in Britain. We find references occasionally to "epidemics" of puerperal fever, but the term as used in England did not connote atmospheric cosmic telluric influences: it indicated rather the occurrence of a considerable number of cases within a certain area and limited to a more or less definite period of time.

The chief factor in the exclusion of the epidemic theory in England was the early participation in midwifery work by English medical practitioners, and the ejection of midwives from their position of privilege, amounting almost to monopoly, in their calling.

According to contemporary evidence the practice of midwifery by midwives in London early in the eighteenth century must have been deplorable. In a pamphlet published by Dr. John Douglas, in 1736, there is a graphic account of the practices of midwives, and some excellent suggestions are made for reforms which have not even yet been realised. Why call midwives " Rude, Rough, Negligent, Ignorant, Foolish, Novice, Obstinate, Over-confident, Supine, Unskilful, Conceited, Self-sufficient, . . . when no effort is made to educate them ?"

His own proposals amounted to this : the establishment of a lying-in hospital on the model of that which he had seen in Paris: systematic and clinical teaching: and examination before obtaining a certificate of fitness to practice. In reply to these wise suggestions came promptly the usual torrent of vituperation about "indecent and unjust aspersions": but the real reply was the development of a "Scheme put in Execution for the real Good and Improvement of Midwifery." The history of this movement will be found best and most concisely recorded in Glaisher's[1] work: "Dr. William Smellie, and his Contemporaries," which is largely drawn upon here.

Smellie has been called the Founder of instruction in Midwifery in England. He was not the first chronologically, but he was by far the most influential in laying the foundations of progress.

The first public teacher of midwifery in England was Dr. John Mowbray or Maubray, whose work "The Female Physician" was published in 1724. v. Siebold says of this work: "It is distinguished from those of its epoch which were so dry and so impregnated with the dust of the schools."

Sir Richard Manningham was just beginning to teach midwifery at the time when Dr. John Douglas's pamphlet appeared, and it may be supposed he was considerably influenced by the discussion which followed. There was not then in London any lying-in hospital, but in 1739 a ward in St. James's Infirmary, to which Manningham was attached, was set apart for lying-in women, and supported by public subscription. This was the first maternity hospital in the United Kingdom. Manningham, from his powerful influence and his important social position, not only founded this institution, but greatly influenced public opinion in support of ameliorations in the position of poor child-bearing women.

Smellie commenced his career as a teacher of midwifery in London in the year 1741. From small beginnings he gradually acquired a great reputation as man-midwife and teacher, and his influence, combined with that of the other teachers, soon began to be felt in in London and the provinces. The disciples from all accounts could not have been very highly instructed practitioners, but they were vastly superior to their predecessors in practice, who were only called in by the midwives to employ homicidal obstetric implements, as in Continental Europe.

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[1] Glasgow, 1894.

Then arose a furious defensive campaign by the midwives of the metropolis. The vocabulary of abuse was exhausted by vituperation against medical practitioners in general, "the pudendists," and the vilest tirades against Smellie in particular. The reader who is interested will find all he need care to know about this warfare in the work of Glaisher already referred to. One reads details with aversion, and yet there was a great principle at stake. It took many years before the questions at issue were finally settled, and the principle of ordinary midwifery practice by medical men was triumphant. Perhaps the victory of the medical profession was too easy, and the rout of the midwives too complete. We owe to the success of the medical practitioner in the struggle to assert his right to practice routine midwifery, the spread over the country of fairly efficient midwifery practitioners, educated to the extent of making exact observations, and publishing their experience for the advancement of obstetric science and practice; but we owe to it also, on the other hand, the neglect of the thoroughly efficient clinical training of students of medicine in obstetrics, which never developed in England on the efficient Continental model, and we owe to it also the deplorable neglect of education and midwifery training of midwives in England up to the passing of the Midwives Act in 1902.

The establishment of several lying-in hospitals in London in rapid succession during the 18th century, and the foundation of the Rotunda Hospital of Dublin, soon produced a considerable contingent of scientific obstetricians, as the pioneers of progressive midwifery. They wrote books and published innumerable pamphlets in the cause of advancing obstetric science. Many of them gave to the medical world their experience in dealing with puerperal fever, and their opinions on its etiology, prophylaxis and treatment. It is these writings which we have now to consider.

THOMAS KIRKLAND, M.D., a teacher of midwifery in London, was the first man in this world to enunciate the true etiology, the import and the prophylaxis of puerperal fever. The statements from Willis onwards for a century are too vague to be accepted as science. We may presume that Kirkland was a Scotsman from Glasgow or Lanarkshire, attracted to London by the fame of Smellie and the two Hunters. His treatise[1] appeared in the year 1774. The treatise is written with a clearness and vigour worthy of Semmelweis. The relevant points may be concisely stated.

In a case in which puerperal fever arose from infection from a man with a poisoned wound, "their fevers both arose from the same cause."

Of a case he says: "I should indeed have been glad if the uterus could have been washed out with antiseptic injections; but this from a variety of obstacles is seldom practised, and I contented myself with desiring the patient to be raised up in bed two or three times a day which seemed to forward the discharge. From this method of treatment I had the pleasure of seeing her recover."

"From all which cases it is evident that an inflammation of the uterus, and a consequent absorption of putrid matter from this part will bring on . . . . puerperal fever, and that the inflammation of the abdomen is frequently the consequence of the fever thus brought on."

Concerning the milk-fever theory Kirkland says: "It is not likely that the fluids which constitute milk should produce any inconvenience by being detained in the habit: if so a milk diet would be dangerous . . . . that part of the blood which becomes milk is not a distinct fluid while it circulates in the blood-vessels: it is therefore as impossible that a fever or extravasations should arise from milk, as that a jaundice should come without bile being first secreted."

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[1] "A Treatise on Childbed Fevers and the Methods of preventing them," by Thomas Kirkland, M.D., 1774.

"If the fever does not give way to these medicines after they have made proper evacuations, may we not conclude that the disease is deeper seated; that the *primæ viæ* are not only affected by putrid matter taken in *ab extra*, but that there has been an absorption of putrid matter from the uterus?"

On the prevention of childbed fever, Kirkland says: "We should always remember that parturition is the work of nature, and when the child presents itself right . . . . he certainly acts most consonant to right reason who leaves the most to her care. . . . I have often wondered why so vile a practice as that of introducing the hand into the uterus . . . . to extract the placenta could ever have taken place. . . . There can be no doubt that more danger is to be apprehended from a hasty bringing away of the placenta than any other part of the process in delivery, not only because small portions may be left behind capable of bringing on a putrid fever, but because more room will be left in the uterus for blood to lodge and coagulate and become putrid. . . . The immediate extraction of the placenta was the practice in fashion when I was a pupil of Dr. Smellie: I think I have seen it end fatally. . . .

"A day or two after delivery, the patient should sit up in bed . . . . by which means I have seen future mischief prevented by the coming away of the coagula. . . . . Some advise pressure above the os pubis for this purpose . . . . one or both of these methods should be used. . . .

"The patient should be kept cool and clean: for I observe that the brown ichor, which finishes a regular discharge of the lochia, never brings on a fever when it is timely removed, and the patient breathes a pure cool air . . . . the very great degree of putridity . . . . is chiefly accomplished by heat among the linen after it is discharged.

"*Upon the whole it appears that puerperal fevers are those only which arise from an inflammation of the uterus; from the abdominal viscera being inflamed in consequence of a hasty delivery; from the absorption of putrid blood or other putrid matter from the uterus; from the coming of the milk; from an inflammation of the breasts; from the absorption of acrid milk, and from a retention of excrement. Epidemic or hospital fevers are fevers, which take their rise from diseases foreign to the pregnant or puerperal state, are only adventitious diseases happening to lying-in women.*"

The editor of the collected series of monographs in which Kirkland's Treatise is included, Dr. Fleetwood Churchill, writing in 1849, bears curious testimony to the singularly enlightened and advanced opinions of Kirkland. He says in an editorial note: "It is remarkable how very near Dr. Kirkland has arrived at the modern conclusion respecting the local disease in puerperal fever;—he includes all in fact, although he does not specify them, and he seems to have had a clearer notion of inflammation (uterine phlebitis) resulting from absorption of deleterious matters than any of his predecessors."

But the truth of the matter is that Kirkland was nearer to the modern position in 1774 than Churchill in 1849. Churchill says in his "Epidemics of Puerperal Fever": "I must honestly avow that whilst I fully admit the existence of local disease, I think that epidemic puerperal fever is something more than that, although I may not be able to define exactly what it is" (p. 34).

Here we have once more the *divinum aliquid*, the mysterious inexplicable element without which the pathology of puerperal fever would be too simple. This was one objection raised to the Semmelweis Doctrine of decomposed animal-organic matter, and its destruction by such ridiculously simple means as with a piece of *Chlorkalk!*

We do not seek to detract in the slightest degree from the merits of Semmelweis: his discovery was all his own and complete. It came in the fulness of time, and found some prepared and receptive minds. Kirkland's opinions were of premature birth, and were permitted to perish in an unprepared and non-receptive world.

It would serve no important purpose relevant to the present subject to go into detail regarding the work of the contemporaries of Kirkland, and his immediate successors. We trace the gradual evolution of the theory of contagion, but there is no prominent treatise on puerperal fever till we reach Gordon, of Aberdeen, at the end of the century.

DENMAN, whose "Essay on the Puerperal Fever" appeared in 1768, added nothing material to the sum of the common knowledge except in methods of treatment. Most of his original observations have not stood the test of time. For examples of his opinions good and bad may be cited: "Rough treatment of the os uteri at time of labour . . . violent or hasty separation of the placenta will often give rise to this disease. . . . Any disturbance raised in the constitution will affect parts already in a very irritable state from the violence which they have so lately undergone. . . . This disease may be sometimes foreseen in the time of pregnancy by an uncommon degree of fever. . . ." Abscesses in the breasts "prevent more grievous and dangerous complaints. . . ."

As is well known, Fochier has tried recently to arrest the progress of septicæmia by the artificial production of abscess. We shall find other contemporaries of Denman who refer to the prognostic import of abscess.

In a "Treatise on the Puerperal Fever, etc.," by NATHANIEL HULME, M.D., which was published in 1772, the author, who has been much quoted, enunciated a doctrine which was neither new nor true. "But I am clearly of opinion that the puerperal fever is as much an original or primary disease as the ague, quinsey, pleurisy. . . . I appeal to dissections. . . . .

" . . . The immediate cause then of the puerperal fever is an inflammation of the intestines and omentum.

Semmelweis in his doubts and difficulties, and Tarnier after him made " appeal to dissections " and received no enlightenment. As well appeal to microscopic sections of a chancre for light on the essential nature of syphilis.

" Practical Observations on the Childbed Fever," by JOHN LEAKE, M.D., Physician to the Westminster Lying-in Hospital, 1772, contain the germs of many opinions and theories which were destined to grow into prominence. "There is perhaps not any malady where powerful remedies of every kind have been tried with more diligence and less success." The disease is an "Acute fever, peculiar to women after delivery." There are two kinds: the putrid and the inflammatory. Atmospheric changes have considerable influence in producing the disease. "All the circumstances attending lochial discharge exactly correspond to receiving a fresh wound. . . . The fever brought on by an inflammation of the uterus has often been confounded with the childbed fever; but those diseases are very essentially different." We shall hear something like this from Scanzoni three-quarters of a century later; and once more what about the priority of Cruveilhier on the placental site as a wound? Leake considered early and copious bleeding at the first onset the best treatment. This was the treatment specially recommended and adopted by Gordon, of Aberdeen, many years later.

The work of CHARLES WHITE, of Manchester, on puerperal fever, published in 1773, is worth recalling at the present time if only for one point alone, his prophylaxis. " One gentleman deservedly of high character in the profession . . . had declared that in his own practice he has seen more frequent instances of the puerperal fever from early sitting up than from all other accidental causes united. Were this, however, the real cause of puerperal fever, it would be astonishing that any of my patients should escape them, as I constantly direct them to sit up in an hour or two after delivery, and to repeat it as frequently as possible, and even to get out of bed in less than twenty-four hours. . . . Puerperal fever has very rarely occurred among those whom I have delivered, and has never once proved fatal."

On this question of early getting up, the Editor, Dr. Churchill cannot restrain his feelings: he says in a note, "It would be wrong not to enter my earnest protest against such rash and dangerous practice."

Early getting up as we have said has recently been revived in Germany: there is no doubt much to be said on both sides. With our lying-in hospital patients, however, the puerperium is the only rest they ever get!

Dr. Walsh published "Practical Observations on the Puerperal Fever" in 1787. Some of his remarks are worth quoting as they mark the development of opinion towards contagionism, and indicate the source of some matter published later by others without due acknowledgment. "I will venture . . . . to assert that the disorder is not one *sui generis* confined to lying-in women, but merely an unusual form of a very common disease." It is an infectious fever complicated with a more or less extensive inflammation of the peritoneum. *The disorder is to a certainty infectious.* Common synochus or fever may cause puerperal fever. Illustrations are given in support of this opinion. The two first women delivered in the lying-in ward, notwithstanding all possible care taken in cleansing and preparing it six weeks previous to the reception of any, were attacked with the puerperal fever to a very violent degree from the common infection (of synochus) being introduced in the clothes of one of the patients; and in another instance "*I was enabled to foretell the occurrence of the puerperal fever* from a putrid fever obtaining in a particular house at the same time that a woman lay-in there."

Walsh said the two fevers arise under similar circumstances. . . . "They rage most during the warm summer and autumn months, and particularly among the lower and more uncleanly sort of women."

On the difference of opinion as to whether puerperal fever is erysipelatous or phlegmonous, Walsh merely remarks that the question has not been rightly settled among the authors on this subject.

GORDON, of Aberdeen, owed his fame chiefly to the notice taken of his treatise by Oliver Wendell Holmes. The essay was entitled: "On the Epidemic Puerperal Fever of Aberdeen," and it was published in 1795.

In the early portion of the article this passage occurs: "By observation I plainly perceived the channel by which it was propagated, and I arrived at that certainty in the matter that I could venture to foretell what women would be affected with the disease upon hearing by what midwife they were to be delivered, or by what nurse they were to be attended during their lying-in; and in every instance my prediction was verified."

This is the passage which O. W. Holmes apparently considered of such weight that he quoted it in capital letters in his address on the "Contagiousness of Puerperal Fever," published in 1843.

On the nature of the disease Gordon says: "Some maintain that the puerperal fever is a disease of an inflammatory, while others as strenuously contend that it is of a putrid, nature. . . . "Since the state of child-bed is the conclusion of a great process . . . and since an inflammatory disposition of body attends the whole process from beginning to end, is it reasonable to suppose that there would be a sudden change from inflammatory to putrid at the close of the process? . . .

"I shall therefore relinquish reasoning and have recourse to facts. . . . It may be considered as an established truth that the puerperal fever is a disease of an inflammatory nature. . . . Gordon continually mistakes a strongly stated opinion of his own for a proof. He goes on: "Having proved that the puerperal fever is an inflammatory disease I shall next endeavour to investigate the specific nature of the inflammation, or inquire whether it be of the nature of phlegmon or erysipelas. . . . I will not venture positively to assert that the puerperal fever and erysipelas are precisely of the same specific nature; but that they are connected . . . and that they are concomitant epidemics I have positive proof. . . ." A curious and familiar interpretation of the cutaneous phenomena, now recognised as septic, occurs in the passage: "The same connection is evident from this circumstance that a very frequent crisis of the disease is an external erysipelas: which is a proof that there is a metastasis or translation of the inflammation from the internal to the external parts."

On the pathology: "The dissections which I have made prove that the puerperal fever is a disease which principally affects the peritoneum and its productions and the ovaria."

On the cause of the disease we may quote the following passages: "That the cause of this disease was a specific contagion or infection I have unquestionable proof. . . . I had evident proofs of its infectious nature, and that the infection was as readily communicated as that of small-pox or measles. . . . I had evident proofs that every person who had been with a patient in the puerperal fever became charged with an atmosphere of infection, which was communicated to every pregnant woman who happened to come within its sphere. . . . This is not an assertion but a fact. . . . These facts fully prove that the cause of puerperal fever . . . was a special contagion or infection altogether unconnected with a noxious state of the atmosphere."

With regard to the most effectual means of preventing infection from being communicated, Gordon holds the same views as the contagionists before and after his time. He cannot say whether the infection of puerperal fever can be destroyed by the same means as that of other fevers, but he thinks it probable. He speaks of purification of infected chambers, fumigation of infected apparel. "The patient's apparel and bedclothes ought either to be burnt or thoroughly purified. The same rule must apply to nurses and physicians. Fresh air and cleanliness are insufficient for the destruction of contagion: there is no certain antidote but fire and smoke.''

The actual expressions employed by Gordon have been given to a large extent so as to enable the reader who has not access to the original treatise, to form some judgment of the writer's mental motions and temperament. He evidently does not reach a high standard of the scientific intellect, and his observations by post-mortem examination, on which he forms such extremely confident conclusions, could only have been the amateur dissections on which, even eighty years later, Matthews Duncan passed well-merited criticism as misleading or useless.

Dr. Gordon's etiology is contagionism in its most crude and uncompromising form; and his contribution is consequently of very little value towards the advancement of obstetric science. We have seen that Semmelweis was misrepresented and belittled by opponents who did not obtain a knowledge of his opinions by tolerably conscientious reading of his works: we cannot but think that at the other extreme and contrariwise, Gordon has been unduly praised by patriotic friends, equally unacquainted with his real opinions and the value of his one contribution to obstetric literature.

It is surely a perfectly fair inference that if Oliver Wendell Holmes so admired Gordon's opinions as to quote even a random detached passage, and to have it printed in capital letters, that the contagionism of Gordon was accepted by Holmes as the true etiology of puerperal fever.

The writings on puerperal fever in the 19th century, up to the time that the Semmelweis doctrine was first announced in England, are, for the most part, mere records of outbreaks of puerperal fever, and guesses at truth in interpreting the import of the phenomena. Not only teachers of midwifery, but observant general practitioners all over the country, published accounts of their experience, and thus accumulated evidence in support of the universally accepted doctrine of contagion. The etiology accepted by the whole profession was contagionist, but the prophylaxis was as thorough and effective, as if the doctrine of conveyed infection by deleterious matter from without, had been adopted decades before it was enunciated and taught in the schools.

In illustration of a type of experience let us take that of Dr. LABATT who was master of the Dublin Lying-in Hospital from 1814 to 1821. In a report to the Board of Governors he explains that the beginnings of unfavourable results in 1818 were owing to the prevalence of typhoid fever in the city. Patients were ill on admission to the hospital, and many of them sank immediately after delivery. By thorough whitewashing and cleansing, the progress of the puerperal fever was arrested. From the 1st of September to the 31st of December, 1819, of 1,010 admitted 129 took the fever and 61 died.

Every possible measure was adopted to arrest the progress of the disease: isolation of the sick, strict attention to cleanliness and ventilation, frequent change of bed and bedding; then scouring, fumigation and whitewashing; painting and whitewashing repeated; furniture and utensils changed; nurses and servants instructed with regard to cleanliness; fumigation of the hospital with chlorine gas and washing with solution of chloride of lime; attention to temperature of the wards.

From the 1st to the 31st of January, 1820, of 171 patients admitted, 63 were attacked and 25 died.

Meanwhile Dr. Labatt and his assistants were occasionally called upon to attend cases occurring "among the lower orders through the city." No wonder that the Master came to certain conclusions including (1) the contagion of typhus fever was capable of giving rise to puerperal fever; (2) puerperal fever was communicable from one patient to another; (3) that it could be carried from the sick by an attendant to healthy lying-in women.

The results of postmortem examination were identical with those ultimately found and described by Semmelweis.

This summary of Labatt's report is given in illustration of hospital experience, but there was gradually accumulating a great amount of experience and observation in private practice, both in towns and rural districts, which was recorded, in contrast to the fruits of the practice of midwifery all over Continental Europe. There only directors of Clinics contributed to the literature of the subject. Exceptions were rare.

One question which constantly recurred was as to the relationship between puerperal fever and erysipelas. Many observations appeared to point to the identity of the two diseases, and Nunnelly, who wrote on erysipelas in 1841, went the length of declaring the question of identity settled.

Among those who early in the century influenced professional opinion most were Robert Lee, Ramsbottom and Copland. They gathered evidence from their own experience and that of others, and each of them laid down rules for preventing the spread of the disease, on the assumption that the disease was contagious and could be conveyed by a third person. Copland in 1833 laid down a series of such rules with a confidence not hitherto expressed. He declared that a medical practitioner was not justified in making a postmortem examination of a person who had died of puerperal fever, of erysipelas or peritonitis or of any diffuse inflammation of cellular tissue or, generally speaking, when death was the result of an animal poison; he should not visit such a class of cases without changing his clothes, and thoroughly disinfecting immediately afterwards; and it would be better if he gave up midwifery practice for a few days when such septic cases had to be attended to.

Although everybody in England has accepted the doctrine of conveyed infection and wound fever, we still find some remnant of the Copland type of contagionism and irrational practices founded on it. Recently in

Wales the Executive Officer of a Local Authority suspended a midwife for three months because she had attended a case of puerperal fever!

The various incidents which swelled the evidence apparently in favour of the contagionist doctrines may be found recorded in the contemporary journals, or collectively in Churchill's work, or perhaps best of all in the translation of Hirsch's Geographical and Historical Pathology. The experience of Paley, of Halifax, 1839, and of Storrs, of Sheffield, in 1841, is especially impressive. Storrs was so impressed with the cases which occurred in his own practice that he set to work to collect evidence from his friends and neighbours, just exactly as O. W. Holmes did years afterwards, and he ultimately formulated certain conclusions. The chief of these were: (1) that puerperal fever may be communicated by touch; (2) that it originates in some animal poison, especially from erysipelas and its complications, and in a less degree from typhus fever.

Storrs prepared a set of rules for the guidance of the medical practitioner, amounting to avoidance either of cases of erysipelas, or of midwifery work, or if both continued in his practice, then the most rigorous use of antiseptics, changing of clothes and avoidance of post-mortem examinations. These rules were almost identical with those of Copland published ten years before.

Enough has been already said to make it clear that the medical profession in England had come very near to the most modern practice in relation to puerperal fever. Their theory of contagion was erroneous, as we now know, but their prophylaxis was excellent; they did, as Semmelweis said, something that was superfluous but not injurious, but the spanning of the narrow channel between advanced contagionism, the conveying of an entity called a disease *qua talis* instead of by a decomposed animal organic material produced by the disease, could not be far off. Black, of Edinburgh, had already called attention to the practitioners' finger-nails as possible vehicles of infection when making vaginal examinations, and almost simultaneously Storrs had also declared his conviction that puerperal fever could be conveyed by touch. It only required the first revelations of bacteriology to bring British obstetrics to the conception of wound-fever.

Hence it was that the medical profession of the United Kingdom was in such a condition of preparedness and receptivity of mind to welcome the news which Routh brought from Vienna. That with the introduction of anæsthesia and the careless and faithless employment of antiseptics there was sad retrogression in practice we can only recognise and deplore.

AMERICA.

In pursuance chiefly of the question of the priority of O. W. Holmes, let us look at the American contributions to the etiology of puerperal fever.

In the United States of America the contemporaries of Semmelweis made but few contributions to the literature of puerperal fever, and these were of little importance.

In 1842 MEIGS, of Philadelphia, who appears to have held rather eccentric opinions upon most things obstetrical, including fierce opposition to the employment of anæsthetics during labour, published a treatise on the pathology and treatment of puerperal fever; but it contained nothing original. He had come largely under French influence. As it included articles by Gordon of Aberdeen, Armstrong, Lee, and others, the work of Meigs may possibly have been the means of first calling the attention of Oliver Wendell Holmes to Gordon's account of the epidemic of puerperal fever in Aberdeen in 1785 which impressed him so deeply. Meigs did not at first believe that puerperal fever was contagious, infectious, or communicable in any way, and in a later treatise he defined it as "a group of diverse inflammations within the belly."

In 1854, Meigs published a second contribution on puerperal fever: it was entitled "On the nature, signs and treatment of Childbed Fevers," and in 1855 a magazine article on the contagiousness of puerperal fever gave a summary of the treatise, which must have reached many who had not seen the book. In the retrospect of the incidence of the disease, the author mentions "the recorded opinion of Rokitansky and Semmelweis," a statement suggesting somewhat inexact information, but he states the Semmelweis doctrine quite fairly. He has evidently read all the evidence in favour of the contagionist theory published in the United States and Great Britain, but he remains in "invincible ignorance." "I have practised midwifery for many long years. I have attended some thousands of women in labour, and passed through epidemics of childbed fever, both in town and hospital; I have made many researches of childbed fever . . . Still, I certainly never was the medium of its transmission." Speaking of a physician of Philadelphia who had puerperal fever in his practice, Meigs says: "Did he carry it on his hands? But a gentleman's hands are clean. Did he carry a nebula or halo about him? Then why not I also?" *Ex pede Herculem.*

Then we have the terrible experiences of Dr. D. Rutter eloquently detailed in support of opinions diametrically opposite to those which they appear to the reader to teach.

After all comes the inconsistent conclusion: "We feel compelled by the evidence on record to admit the possibility of puerperal fever being conveyed . . . by those who attend midwifery cases after being employed in dissection or postmortem examinations." Under ordinary circumstances, says Meigs, we are not justified in attributing the spread of the disease to contagion.

Here we have also incidental proof that the Semmelweis doctrine had reached Philadelphia before 1854, six years before the publication of the *Aetiologie*; it was spreading really on its merits while Semmelweis stagnated in Buda-Pesth, without raising his voice against Continental detraction.

Kneeland's contribution (1846) was of a different stamp. It contained at least his own observations, and some account of the experience of other American practitioners, but it was exactly like the contributions to the subject, such as those of Armstrong, Lee, Ingleby, Roberton, Blackmore, and Storrs, and a host of others, which had been appearing in England from the beginning of the nineteenth century and even earlier, as for example in the work of Charles White of Manchester which was published in 1773.

Kneeland maintained that puerperal fever was contagious, and that it is propagated from one patient to another in the wards of a hospital. Epidemics of puerperal fever are almost always the effect and not the cause of the contagion.

OLIVER WENDELL HOLMES. But all American contributions of that time have been overshadowed in recent years by the factitious importance conferred upon the essay of Dr. Oliver Wendell Holmes on the "Contagiousness of Puerperal Fever." It was published in an evanescent medical journal in New England in 1843. If the late Dr. Cullingworth[1] had not conferred distinction upon this essay by an admirable address on the subject, comparatively few medical men in England would have remembered that Holmes belonged to their profession. His contribution would have taken its due place in the European histories of obstetrics in a mere catalogue of names, as exemplified in the work of Hirsch and of v. Winckel and others. He is not even mentioned in the "History of Obstetrics" by v. Siebold or in the continuation by Herrgott, and there is no sufficient reason why he should have been.

Dr. Cullingworth was a patriotic Englishman, and a well-read, cultivated, and honourable man, so that no question of personal or national bias can be entertained in connection with his work; none of the emotional preconceptions which for example disfigured the controversy in Germany about the work of Semmelweis.

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[1] Oliver Wendell Holmes and the contagiousness of puerperal fever. —*Brit. Med. Journ.*, 1905.

But Dr. Cullingworth had a fine literary taste and he was a great admirer of the "Autocrat," as a man of letters; and it is therefore just possible that he carried his admiration of Holmes, as poet and essayist, unduly to his credit as a member of the medical profession. So it came to pass that the reputation of Holmes was unduly raised because of his literary genius, whilst Semmelweis was unfairly depreciated because of his exaggerated sense of unfitness for literary work.

If Semmelweis could have written like Holmes, his *Aetiologie* would have conquered Europe in twelve months.

Holmes was not an experienced medical practitioner at the time when the after-meeting conversation at a Medical Society in Boston turned his attention to puerperal fever. The story of conveyed infection which he then heard shocked him, and he concluded that it would be a good thing to let the medical profession in general know something of what he had just learnt. So he set to work to obtain by correspondence the personal experiences of men whom he knew, and to read the published records of puerperal fever in America and England. The thesis which he had to discuss was well worn before his time: "The disease known as puerperal fever is so far contagious as to be frequently carried from patient to patient by physicians and nurses." Holmes passed in review the work of others: he did not contribute a fact of his own personal experience, or an original idea towards solving the problem of causation. He was still, after all that had been written in the course of half a century, as much a contagionist as Gordon himself, whom he quoted with deep solemnity, and he even printed a quotation from Gordon in capital letters. To the English evidence with which we are familiar, he adds the reports of some American practitioners, such as Peirson of Salem, and relates the case of the unfortunate Rutter of Philadelphia; but these are exactly like the reports of such men as Armstrong of Newcastle, whose tales had been told and published decades before. He speaks of a woman as " immersed in the virulent atmosphere of an impure lying-in hospital or poisoned in her chamber by the unsuspected breath of contagion." He speaks later of " the undisputed fact that within the walls of lying-in hospitals there is often generated a miasma palpable as the chlorine used to destroy it." This is probably the poetic rendering of Copland's prose essay. Of miasma, in the obvious sense implied, Semmelweis denied the existence; if Holmes means the "puerperal Geruch" of v. Arneth, then the language is mere rhetoric, but very telling and useful rhetoric.

Here then is the sum of the services of O. W. Holmes to obstetric science: as science it is a neglectable quantity. But that Holmes conferred immense benefits on humanity by devoting his literary genius to attracting attention to puerperal fever, and by trying to suppress the practices which brought childbed fever in their train, is a fact which shoutld be gratefully acknowledged.

But how, in the name of historic truth, does all this bring him into any sort of conflict or even comparison with Semmelweis? The eulogists of Holmes have compared unlike things. All that Holmes wrote was true, as case records, though not much of it was new; apart from cases he only restated in eloquent language the old and obsolescent opinions.

Still, it brought down upon him the most truculent attacks from obscurantists in the highest official positions. Hodge, Professor of Obstetrics in the University of Pennsylvania, attacked Holmes with a certain amount of dignity not unworthy of the subject, but Meigs, Professor of Midwifery at the Jefferson Medical College of Philadelphia, assumed the old aboriginal American style of warfare, and attacked him with the tomahawk and scalping knife of the Red Indian savage. He astutely hit the taste of his fellow-countrymen, their gambling propensities and their religious sentiment, by attributing puerperal fever to "chance or Providence." But it would not be for edification to enter into a controversy which has been overworn and made obsolete by the progress in knowledge of over half a century. It was at its best only a dispute between *laisser faire* and the barren doctrine of contagionism—the conveyance of a disease *qua talis*. We have no hint in it of the conception of Uebertragbarkeit.

One point might receive attention in view of the absurd claims of priority of a discovery on behalf of Holmes. In spite of the earnestness of his appeal, we are haunted with the sense of unreality: he is a barrister who has received his instructions, and is doing his eloquent best, with his brief in his hands, and his junior within earshot: "I do not expect," he says, "ever to return to this subject." To Semmelweis "this subject" was his whole existence; he worked at it and he fought for it to the last, and he left it only to descend to the tomb.

The sense of unreality, instead of devouring earnestness in the advocacy of such an immeasurably important cause, is produced again by a certain affectation at times such as in the dispute with Meigs: "No man makes a quarrel with me over the counterpane that covers a mother with her new-born infant at her breast." Surely the controversy was too desperately serious for such refined sentimentality.

Holmes becomes more attractive when he says: "Let it be remembered that *persons* are nothing in this matter; better that twenty pamphleteers should be silenced, or as many professors unseated, than that one mother's life should be taken. There is no quarrel here between men but there is deadly incompatibility and exterminating warfare between doctrines." But was not warfare to be carried on by articulate language suitable to the struggle? Holmes, although he had no practical experience, drew up a set of admirable rules for medical practitioners in order to prevent the spread of contagion, but they might have been copied from the rules of Lee, or Copland or Storrs, all published years before it was known in England that Dr. Oliver Wendell Holmes had turned his attention to puerperal fever.

We feel confident that if the medical profession in

America had placed before them a fair and unemotional statement of the history of the Holmes episode, and a concise history of the services rendered to obstetric science by the leading contagionists in Great Britain, such as Lee, and Copland and Storrs and any number of Masters of the Rotunda Hospital, their sense of justice would bring the question of the priority of Holmes to an end. No question can be settled by ignorance and prejudice. The discussion about contagionists does not touch the position of Semmelweis. He brought a new idea into the world, and by his genius he solved the problem of Etiology once and for all time.

The work of Dr. Whitridge Williams[1]: "A Sketch of the History of Obstetrics in the United States up to 1860," which has been laid under contribution to some extent in the preceding pages, is a perfectly just and unbiassed record of facts and opinions, still it might be permitted without offence to suggest in reference to it the English cricket umpire's definition of his duties: "Fairation for ever with a leaning to your own side." We cannot expect that the same trait will not be found in the present treatise, whatever the principles laid down at the commencement as absolutely binding on the writer.

When Dr. Whitridge Williams next makes a study of the history of midwifery, if he would take the subject of puerperal fever in the century up to 1860, trace the services of the British contagionists, and fully weigh the import of the new evangel of Semmelweis, we feel confident that he will recommend his countrymen to abandon the medical claims advanced for Oliver Wendell Holmes, and advise them to rejoice only in the splendour of his literary genius.

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1. *American Gynæcology*, 1903.

IX.

Antisepsis in Midwifery.

ANTISEPSIS AT BUDA-PESTH.

v. Waldheim says that v. Balassa, the Professor of Surgery, who held such a distinguished position throughout Hungary both on account of personal and professional qualities, did not think it worth the trouble to adopt the antiseptic method of Semmelweis. But this appears to be a mistake. Tiberius v. Gyory states that the incident of v. Balassa assisting Semmelweis at the first ovariotomy performed in Hungary, in 1863, was not merely an isolated act of collegial courtesy, but that v. Balassa himself rigidly applied the Semmelweis antiseptic method in his surgical work (*Sondern ein jedes Mal auch die dort gelehrten antiseptischen Massregeln genau befolgte*). v. Gyory possesses contemporary evidence in writing that at least from 1858 onwards in Hungary the chlorine disinfection was rigidly practised not only in the Obstetric Clinics but also in the Surgical.

This fact is of remarkable interest historically inasmuch as it proves that before the work of Pasteur was known, and before Lister introduced his methods of preventing wound-fever, and long before anyone elsewhere thought of routine antiseptic midwifery, the systematic use of antiseptics in midwifery, gynæcology and surgery had been regularly practised in Buda-Pesth.

The antiseptic employed was chloride of lime, but there can be no doubt that with further experience some of the newer chemical substances would have been brought into use. So we may without violence claim for Semmelweis that it was he who introduced antisepsis as a prophylactic measure both in obstetrics and gynæcology.

Bacteriology and its Practical Applications.

The supplementary knowledge which Markusovszky prophetically declared to be essential to complete the understanding of puerperal fever was soon to be revealed.

Among the most obvious advances in Science relating to puerperal fever was the foundation of Bacteriology. Even at the time when Markusovszky was writing, Pasteur was at work, and had already discovered the streptococcus; and only two years later appeared the first contribution of Mayrhofer, assistant to Carl Braun of Vienna. Articles of Mayrhofer's were published in the Vienna medical press for the next two or three years. He had discovered vibriones (bacteria) in the air of the lying-in hospital wards, and then in the lochia of sick puerperæ, and he finally reached the conclusion that the examining finger, not the atmosphere, was the usual carrier of the vibriones into the female genitals, and that the air was innocuous, just as we find later that owing to the same belief the spray and other appliances introduced by Lister for the destruction of living organisms in the air came to be looked upon as superfluous. It is not without interest in this connexion to recall that it was Lucretius: *De Rerum Natura,* who first called attention to the bodies which produce the opacity in a ray of light entering a darkened room.

Contemplator enim, cum Solis lumina cunque
Insertim fundunt radios per opaca domorum;
Multa minuta modis multis per Inane videbis
Corpora misceri radiorum lumine in ipso.

. . . . . .

For Lucretius these *corpora* were the *primordia rerum* with the potentiality of development.

With perhaps less internal evidence of observation and reflection, claims of priority for contributions to medical science have been put forward in more modern times.

Soon after the publication of Mayrhofer's last article, Dr. Hausmann discovered (1868) vibriones in the lochia of perfectly healthy puerperæ, and in the vaginal secretion of pregnant women. Therefore, it was argued, the pathogenic nature of vibriones was disproved.

This episode was the worthy beginning of the vast volume of bacteriological observations connected with midwifery, and of the controversy of the last forty years. The very latest question raised in the confused tug-of-war is with regard to the hæmolytic influence of the streptococcus, on which contradictory opinions are put forward, and maintained, and defended, with the confidence characteristic of Bacteriology. The opinions expounded appear to depend largely upon the particular school or hospital whence they emanate.

Pasteur discovered the streptococcus in a case of puerperal fever in 1860, and Ogston, of Aberdeen, when most eminent surgeons became keenly interested in the scientific basis of their practice, was among the first to identify the micro-organism in surgical pyæmia.

The great event after the publication of the work of Pasteur was the epoch-making address of Professor Lister, of Glasgow, "On the Antiseptic Principle in the Practice of Surgery," in August 1867.* It was the result of years of experiment and reflexion frankly based on the work of Pasteur. His aim was to prevent the access of the disease-bringers.

Lister said: "But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended, not on the oxygen or any gaseous constituent, but on minute organisms suspended in it which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding the air, by applying as a dressing some material capable of destroying the life of the floating particles . . ." "The material which I have employed is carbolic or phenic acid" . . .

. . . Since the antiseptic treatment has been brought into full operation, and wounds and abscesses no longer poison the atmosphere with putrid exhalations, my wards . . . have completely changed their character, so that during the last nine months not a single instance of pyæmia, hospital gangrene or erysipelas has occurred in them."

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* *Brit. Med. Journal* and *Lancet.*

We need not dwell upon some opposition to the Listerian doctrine and methods, chiefly on the part of his own countrymen, among whom it is saddening to find the name of Sir James Simpson. But the opposition did not last for thirty years as in the cases of Harvey, Jenner and Semmelweis. Upon the whole, it may be said that the adoption and success of Lister's methods were immediate and universal, partly one of the fruits of the vast extension of medical journalism in the years since the Semmelweis discovery was first so defectively announced. While the knowledge of Lister's work was spreading over Europe, some notable contributions to the bacteriology of puerperal fever became more or less widely known.

Coze and Feltz, of Strassburg, inspired by Pasteur's communication to the *Académie des Sciences,* and his *Etudes sur le vin et ses maladies,* published in 1865, set to work on "ferments," and investigated the fluids obtained in cases of puerperal fever. In 1869 they published some results in their local medical journal. In one case which was very severe and ultimately fatal, they discovered bacteria in the blood of the living woman: "Ce sang était caracterisé par une quantité très considérable de leucocytes . . . et la présence de nombreux points mobiles isolés ou disposés en chainettes." The work of Coze and Feltz was interrupted by the war, and it was not published so as to become widely known until several years later.

The Listerian dressing of wounds was introduced into France comparatively early by Lucas-Championnière and Guyon, and in 1876, nine years after Lister's historic address, the practice of applying external antiseptic dresing to the vulva after labour was commenced in the lying-in hospitals of Paris. Vaginal and uterine injections with carbolic acid solution were first employed two years later.

Among the most notable of the early contributions to the bacteriology of puerperal fever was that of Waldeyer of Breslau, which appeared in 1871. It was entitled:

"On the Presence of Bacteria in the Diphtheritic Form of Puerperal Fever." He found bacteria on the internal surface of the uterus, in purulent masses in the lymphatics, in the broad ligaments, and in the serosity of the peritoneal cavity.

Spiegelberg, professor of midwifery, in speaking of his colleague's observations said: "There are two analogous maladies: traumatic fever as found in the surgical wards of hospitals, and puerperal fever in the lying-in hospitals. This last is a putrid absorption, a septicæmia. The traumatic surfaces in the genital canal, which are very numerous, are the site through which absorption takes place."

This doctrine had found other individual exponents throughout Europe, but it was in Germany that the doctrine and practice of Lister received the earliest and most enthusiastic reception and support; and to their honour it must be acknowledged that some of the oldest antagonists of Semmelweis were among the first to see the analogy between wound-fever and puerperal fever, and to apply the Listerian principles to the ordinary practice of midwifery, and to the prevention and treatment of puerperal fever. It was not a long step from the recognition of the lesions in septic endometritis to cleansing the "diphtheritic" surface.

We have seen that Kirkland, about 1772, was deterred by the practical difficulties from flushing out the uterus in a case of puerperal fever, which was to him even then wound-fever. Eisenmann, as early as 1837, suggested flushing with sublimate solution, but his advice fell on deaf ears, or he himself did not perhaps reach that firm conviction which embodies itself in strong action.

Stoltz, at Strassburg, used a double-current tube to flush out the septic uterus with chlorinated lime solution, but without very satisfactory results. It was Grünewald, of St. Petersburg, where the Semmelweis doctrine received such a hearty and early reception, who first set to work systematically to flush out the uterine cavity, and he was wonderfully successful considering the small quantity of fluid injected. This was the real beginning of the new era. Grünewald had at once many imitators, and the practice soon became universal.

But all these innovations were only tentatives in the hands of the few experts. It is chiefly to Doléris,[1] whose inaugural thesis was published in 1880, that the merit of popularising antiseptic midwifery in France, and then largely in Europe, is due.

Doléris had the good fortune to work under Pasteur when the master was making observations on puerperal fever at the Maternité in 1879, and Doléris began to study the lochia from the point of view of the presence of micro-organisms and their relation to the symptoms of puerperal fever. The work of Doléris is important only as an exposition of the opinions of Pasteur formed on actual intimate experience of puerperal fever, rather than on account of any fresh discovery. It settled nothing even temporarily, and it has all been corrected or supplemented for many years past.

But Doléris himself continued to be one of the chief contributors to the progress of the bacteriology of the puerperium. We find the fruits of his labour in the discussion on the Etiology and Nature of Puerperal Infections (*Etiologie et Nature des Infections Puerpérales*) at the International Medical Congress in Paris in 1900. In that discussion he took a leading part, and explained the position which had then been attained on the subject in a long and exhaustive address. Among the "reporters" who followed him were Krönig and Menge among the German bacteriologists, and Pestalozza, now of Rome. They were followed, non-officially, by Veit, then of Leyden, and Döderlein, now of Munich, and others. Sir A. R. Simpson, of Edinburgh, made a request for some guidance for the medical practitioner in dealing with an actual case, and he well might do so; but we doubt if he received any reply worth mentioning.

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[1] Essai sur la pathogénie et la thérapeutique des accidents infectieux des suites de couches.

However, we are safe in saying that that discussion brought out all that was ascertained knowledge on the bacteriology of puerperal sepsis up to 1900; what was not expounded, and much that was, may be put down in the category of "not knowledge."

Nothing of essential importance has been added since to this department of bacteriological science.

But the practical applications of bacteriology have steadily grown and expanded and many infected patients, who a few years ago would have been left to perish, are now rescued by manipulation and operative measures largely founded on the results of bacteriological observation and research.

Fritsch mentions the year 1872 as the time when antiseptics were first introduced into midwifery practice in Germany, but the employment of the antiseptic method was certainly far from general then in Germany or anywhere else.

"Who among us," says Fritsch, "has not had experience of the kind of case which lay forever as a burden on our conscience, where we applied the forceps to a strong healthy primipara on account of some lax indication. . . . At the end of five days the woman was dead, died from acute sepsis. . . . The doctor who should have brought help, brought death."

It was Fritsch himself, a man of intellect and human sympathy, who was chiefly instrumental in introducing rational and effective antisepsis into obstetric practice in North Germany. v. Grünewald employed by far too little fluid in his intra-uterine disinfection. Fritsch flushed with a copious amount of fluid and was remarkably successful.

As far as we can learn from searching the literature of the time, Dr. J. G. Wilson, Professor of Midwifery in the Andersonian University of Glasgow, was the first to resort to copious antiseptic flushing of the uterus in imitation of Listerian antisepsis. He published a "Note on Carbolic Acid in Obstetric Practice" in the *Glasgow Medical Journal* of May, 1869.

He said in the course of this paper that it was more than a year since he first employed diluted carbolic acid as an intra-uterine injection after parturition, and he mentioned in some detail the facts of his first case. It was that of a patient showing the symptoms of septicæmia with scanty fœtid lochia and hypogastric pain. The result of the flushing was immediate and satisfactory, successful in every way.

Dr. Wilson says further: "Since then I have frequently syringed the uterine cavity with a weak solution of carbolic acid. . . . That carbolic acid has proved of decided value in many such cases I have not the slightest doubt."

It is much to be regretted that Dr. Wilson was so long in finding imitators or any sort of acknowledgment whatever among his own countrymen. Grünewald, whose process of treatment was begun also in 1868, had imitators in Russia and Germany from the first, and his method was everywhere acclaimed throughout Continental Europe.

The practice of disinfection, both external and intra-uterine, having become completely established, it assumed many forms.

Schede, a surgeon of Hamburg, drained the canal of the septic uterus, but he soon saw reason for giving up the practice. Schücking tried permanent irrigation. Both in prophylaxis and treatment measures were carried to extremes; flushing of the vagina before, during, and after labour; the vagina was packed after labour with antiseptic tampons; some even went the length of imitating Lister's surgical method, and keep carbolic acid spray playing upon the relevant parts during labour. There was too much meddling in matters which had hitherto received too little attention; and slowly came a beneficent reaction.

Hospital practice became rational, conservative and safe; while in the treatment of private patients, both by doctors and midwives, apathy with regard to antiseptic prophylaxis, or feebleness and inefficiency in the application of local measures, amounting to malpractice, with consequent high morbidity and mortality, continued, and in many quarters still continues.

One direct consequence, which we trace to the Semmelweis Discovery, is that the safest place for working-class women to be confined is within a well-conducted lying-in hospital; and of no such institution can that be said with more confidence than of the *Gebärhaus* of Vienna at the present time—the birth-place of the " Doctrine."

CONCLUSION.

We have now completed our task of trying to convey to members of the medical profession who read the English language some impression of the personality of Semmelweis, and some account of his epoch-making discovery, and the spread of his doctrine. We have endeavoured to tell the simple and sad story of his life, and to relate the chief episodes in the controversy concerning the Semmelweis Doctrine until its final triumph.

In the short pathetic *Nachwort* to the *Ætiologie* he wrote: "When I with my present convictions look back upon the Past, I can only dispel the sadness which falls upon me by gazing into that happy Future when within the lying-in hospitals, and also outside of them, throughout the whole world, only cases of self-infection will occur. . . .

"But if it is not vouchsafed to me to look upon that happy time with my own eyes, from which misfortune may God preserve me, the conviction that such a time must inevitably sooner or later arrive will cheer my dying hour."

The Doctrine of Semmelweis did triumph beyond the measure of all his most cherished hopes and anticipations; it is the Doctrine of Semmelweis which lies at the foundation of all our practical work of to-day. Through all the details of prevention and treatment, the temporary fashions and the changes of nomenclature, the principles of Semmelweis have remained our steadfast guide. The discoveries of the bacteriologists from Pasteur onwards, and the work of Lister, only explained and confirmed; they were auxiliary, in no wise conflicting.

The only apparent exception has been in the change of opinion regarding self-infection. Bacteriological observation and research have thrown much light upon the relations of autogenetic and heterogenetic infection as vaguely and variously comprehended from Semmelweis to Robert Barnes and some of his contemporaries. Even still in England especially we find the desire to keep up an unreal distinction, a conscience-salving difference, in spite of the overwhelming evidence that all infection comes from without. The reports from all the lying-in hospitals, as well as the results obtained by the care of women amid the humblest and most unpromising home surroundings, prove to all unprejudiced minds that when the difficult process of Listerian asepsis is thoroughly carried out no residuum or *divinum aliquid* of self-infection remains to cause puerperal morbidity.

To Semmelweis it was not vouchsafed to see the final triumph of his Doctrine. By his intellectual endowments and the motive power produced by the warmth of his desire to benefit Humanity, he was predestined to solve the problems which had baffled the ingenuity and acumen of so many generations of investigators; but the strength of his conviction that his was the "eternally true" etiology of puerperal, and that it would at once spread and universally prevail on its own merits without his paternal care and advocacy, permitted opponents to misrepresent it for many long years. If he had possessed the self-consciousness and personal ambition of some of his antagonists and spread the evangel with the persistency and vehemence of the opposition, he might have seen the triumph of his teaching in his own lifetime, and, like Edward Jenner, earned some of the fruits of success. But he was absolutely devoid of the qualities which express themselves in self-seeking and personal ambition, and hence it was that he did not see the happy time for which he longed. Let us conclude as is most suitable and becoming with a paraphrase of Bruck, the eloquent, sympathetic and just biographer.

We deplore his martyrdom, but we can find some comfort in the reflection, that he did not struggle in vain, and that he did not suffer in vain. The whole civilized world was soon to enjoy the fruits of his immeasurably beneficent discovery. He had thrown the light of scientific progress into a region hitherto shrouded in the darkness of Egyptian night. The great revolution of modern times in Obstetrics as well as in Surgery is the result of the one idea that, complete and clear, first arose in the mind of Semmelweis, and was embodied in the practice of which he was the pioneer. When we with just satisfaction contemplate and enjoy the achievements which with mighty strides bring us nearer to the Fortune's crowning slope of full fruition, every time must the name of Semmelweis be uttered with grateful recognition.

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